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NASA HEADQUARTERS
1520 H STREET NORTHWEST
WASHINGTON 25, D. C.
TELEPHONE: EXECUTIVE 3-3260 TWX: WA 755

December 26, 1961

Dear Dr. DeBra :

Enclosed is a copy of the proceedings of the Conference on Experimental Tests of Theories of Relativity sponsored by National Aeronautics and Space Administration at Stanford University on July 20-21, 1961, under the chairmanship of the late Professor H. P. Robertson, California Institute of Technology. These proceedings were prepared under the direction of R. T. Jones, NASA, Ames Research Center.

Best wishes for the Holiday Season.

Ernst J. Ott for

Nancy G. Roman, Chief
Astronomy & Solar Physics
Geophysics & Astronomy Programs

PREFACE

Experiments in space afford new opportunities for testing theories of relativity and gravitation. Numerous proposals for such experiments have been received by the NASA, NSF, and other government organizations. Such proposals are difficult to evaluate except by those who have devoted long study to these subjects. To cope with this problem, the NASA Office of Space Sciences sponsored a conference on Experimental Tests of Theories of Relativity. The suggestion that such a conference be held was made by Prof. W. A. Fowler of the California Institute of Technology. The conference was arranged by R. T. Jones of NASA Ames Research Center and Dr. N. G. Roman of the Office of Space Sciences.

The conference was held at Stanford University on July 20-21, 1961 and was attended by more than 30 well-known authorities. Professor H. P. Robertson of the California Institute of Technology served as chairman. The meeting produced stimulating discussions of the types of experiments that might be performed and various aspects of current theories that might be tested by new techniques. A rather thorough record was kept of the proceedings of the conference, and is included in the present document.

I wish to thank Mrs. Helen Drew for her effort in securing an accurate transcript of the conference and Mrs. Claire Barskey for assisting with the arrangements. Thanks are also due Mrs. Carol Tinling for her assistance in editing the rather difficult transcript and to Mrs. Nancy Thomasson, Mrs. Sarah Ogata and Mrs. June Zyskowski for preparing the manuscript.

R. T. Jones
Ames Research Center

SUMMARY

A conference on experimental tests of theories of relativity, sponsored by the National Aeronautics and Space Administration, was held at Stanford University on July 20 and 21, 1961. The chairman of the conference was H. P. Robertson (California Institute of Technology), and arrangements were managed by R. T. Jones (NASA). There were about 35 attendees from half as many different institutions, a small enough group so that all sessions could be handled quite informally. Only six papers were prepared in advance, and most of those present participated in discussion of these and related matters. A brief account of the six papers is given below, together with an equally brief mention of a few of the points that were brought out in the discussion.

In his introductory remarks, Robertson stated that NASA had asked those interested in the possible uses of satellites and rockets for testing theories of relativity to hold a conference and advise NASA on the value of various proposals. He expressed the opinion that with present techniques, tests of the special theory of relativity could best be performed on the surface of the earth, and that rockets would be more useful for tests of the general theory of relativity and other possible theories of gravitation. He then reviewed the present experimental basis of general relativity: the red shift follows from more elementary considerations and is not really a test of general relativity, and the deflection of light by the sun has not been measured with great precision; only the precession of the perihelion of the orbit of the planet Mercury provides an accurate test of Einstein's theory, and fortunately this includes the lowest order nonlinearity.

The first prepared paper was given by R. V. Pound (Harvard University), who described his now-famous terrestrial experiments which measured the gravitational shift of Mössbauer radiation. For the available vertical height of 70 ft, the fractional shift is 2.3×10^{-15} in each direction, whereas the fractional line width is about 10^{-12}. In spite of this disparity, the ratio of the observed to the theoretically expected shift now stands at 0.97 ± 0.035. He remarked that the cost of the entire series of experiments was about one percent of that of the fuel for a single large rocket; but he hopes to improve the accuracy by an order of magnitude in any event without interfering with the satellite program.

In the discussion, O. H. L. Heckmann (Universities of California and Hamburg) said that solar red shift measurements cannot be expected to be reliable for the present because of large violet shifts from granulations. On the other hand, terrestrial observations of 40 Eridani B and Sirius B are improving, and satellite observations, both of the red shifts from these stars and of the solar deflection of starlight, offer great promise. J. G. King (Massachusetts Institute of Technology) reviewed the studies that had been made of a possible satellite measurement of the combined gravitational and Doppler shifts. This work stopped about a year ago, and his group has no plans for a proposal for a satellite experiment. N. G. Roman (NASA) mentioned the status of the similar experiments considered by Hughes Aircraft and the National Bureau of Standards, and stated that neither of these is now being funded by NASA. There was general agreement with Robertson's conclusion that a satellite effort is not worthwhile for this experiment.

R. H. Dicke (Princeton University) described various ways in which current ideas about gravitation, which are based on Einstein's theory, might be modified. He stressed the importance of null experiments, such as those of Eötvös on the equivalence of gravitational and inertial mass, which are now being improved at Princeton. He then referred to Dirac's cosmology, with its possible connection between the values of natural "constants" and the age of the universe. A possible new theory, similar to one proposed by Jordan, would replace the Newtonian gravitational "constant" by a scalar field that would depend on the proximity of matter. Some of the predictions made by these theories might be subject to experimental test; however, numerical estimates of the effects to be expected cannot be made with definiteness, since the theoretical parameters are not determined in advance. In the discussion, W. A. Fowler (California Institute of Technology) and Heckmann questioned some of the estimates that had been used for the age of the universe; no conclusion was reached on the connection between the theoretical parameters and existing cosmological observations.

In the third paper, J. Siry (Goddard Space Flight Center) described the minitrack (radio) and optical methods for tracking satellites. The optical system has errors of about 7 inches of arc along the track, and about 2 inches at right angles. The minitrack system can be of comparable accuracy when freshly calibrated and, in addition, gives altitude errors of a few hundred meters and radial + Range [illegible] velocity errors of about 10 cm/sec. C. W. Sherwin (Aerospace Corporation) suggested in the discussion that the motion of a satellite be slaved to a freely falling test object so that the latter is always at the center of the satellite. In principle, this test object could be free of all forces except that arising from the gravitational field of the earth, moon, etc. The satellite would then be constrained to follow its motion, and at the same time would protect it from environmental disturbances such as radiation and atmospheric gas. The trajectory of such a slaved satellite would be truly representative of the gravitational field, and might supply very high quality information from which the mass multipole moments of the earth could be computed. This concept of a slaved satellite was proposed in 1959 by G. E. Pugh.

In a companion paper, J. Mitchell (NASA) described the capabilities of existing and anticipated satellite vehicles. Comparison of a typical current satellite with the OAO (orbiting astronomical observatory) of a few years hence shows an increase in weight from 100 to 1000 pounds, an increase in available electrical power from 10 to 100 watts, and the replacement of single-axis spin stabilization with 10° precision by three-axis stabilization with precision better than 1 ~~foot~~ min of arc. He also emphasized, for the edification of physicists unfamiliar with the realities of satellite experimentation, that conditions are radically different from those that are obtained in a laboratory. The experimenter is dependent on many other persons for crucial components, he must make his plans two years ahead of launching and then meet definite schedules, and he must be prepared for failure to orbit. Frustrations occur repeatedly, but the rewards of a successful shot are high.

The next paper, by L. I. Schiff (Stanford University), described the predictions of the Einstein theory with regard to the motion of the spin axis of a gyroscope that is either at rest in an earth-bound laboratory, or in a free-fall orbit about the earth. In either case, the Newtonian theory predicts no precession of the spin axis if the gyroscope is spherically symmetric, while general relativity theory predicts both the geodetic precession arising from motion through the earth's gravitational field, and the Lense-Thirring precession that represents the difference between the gravitational field of the rotating and the nonrotating earth. If the gyroscope is at rest with respect to the earth, it is carried around the earth once a day by the rotation of the earth, and its weight must also be supported by a nongravitational force; the latter gives rise to an additional Thomas (special-relativistic) precession. In this case, all three terms are of the same order of magnitude, and the total precession is about 0.4 sec of arc per year. If the gyroscope is in a satellite at moderate altitude, the geodetic precession is about 7 secs per year, the Lense-Thirring precession is about 0.1 sec per year, and the Thomas precession is zero.

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The discussion was devoted mainly to two possible satellite-gyroscope experiments. W. A. Little (Stanford University), representing W. M. Fairbank who was unable to attend the conference, described a proposed gyroscope that consists of a superconducting sphere supported stably on a static magnetic field. The difference between the local acceleration of gravity g and the true acceleration of the satellite arises mainly from atmospheric gas and should be of the order of 10^{-7} g at moderate altitudes; this greatly simplifies the problem of supporting the spinning sphere. Ambient electric and magnetic fields can be greatly reduced by using a superconducting shield, and the low temperature required also decreases thermal distortion since all coefficients of thermal expansion are very small. A temperature of around 4° K can be maintained for a year by sublimation of a hundred pounds of solid hydrogen, and an additional five liters of liquid helium would keep the temperature below 1° K. The orientation of the spin axis would be observed by putting a spot of a suitable radioactive material on the sphere, and using the Mössbauer effect to aline the spin axis of a synchronously rotating absorber with that of the sphere. Experiments are under way on all aspects of this system. Pg 88

A different kind of extreme precision gyroscope was described by Nordsiek (University of Illinois and General Motors). This consists of a conducting sphere that is supported by an electric field with the help of a feedback loop. It is in an advanced stage of development, and drift rates are now being held to less than 3×10^{-8} radian/sec when it is supported against normal earth gravity; it is expected that this can be reduced by a factor 30. (Note that 1 ~~inch~~ sec of arc per year is equal to 1.5×10^{-13} radian/sec.) Satellite operation at 10^{-7} g would certainly lower the drift rate by several orders of magnitude. Reading out the orientation of the spin axis is accomplished by an optical method; this can now be done with an accuracy of 0.2 ~~inch~~ sec, and improvement by a factor 10 should be possible. Pg 94

Discussion continued on both the superconductive and electric gyroscopes. The possibility of using a satellite slaved to the gyroscope was also mentioned; this would replace the problem of gyro support by the problems of gyro sensing and satellite control. However, it would also give the satellite a true gravitational orbit, which would be of interest for other reasons. In response to the suggestion that the general relativistic perihelion precession of such an orbit might be measured (as with Mercury), Schiff pointed out that for an equatorial orbit this effect is only about a millionth of the precession caused by the earth's equatorial bulge. Roman remarked that NASA would like to be kept informed of plans and progress along all three of these lines. Reservations for space aboard a satellite cannot be made until an experiment is quite certain to succeed, and then must be made a year or two in advance of launching. In response to a question, she stated that a 36-inch telescope might be in orbit by the end of 1965, and that orientations could probably be held to 0.1 inch.

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The last paper was presented by J. Weber (University of Maryland) on the detection and production of gravitational waves. He first discussed natural receivers, such as the earth and the moon. Excitation of vibration and rotation of the earth by gravitational waves from outside would be very difficult to detect because of the noise arising from winds. The moon would be much better in this respect, and a moon crust strain detector might be a useful object to send there. Laboratory detectors of gravitational waves would best be constructed of piezoelectric crystals operating in high modes; these would also make the most efficient generators of gravitational waves. Preliminary experiments on these are now under way.

There followed some discussion between Weber, Dicke, and P. G. Bergmann (Syracuse University) of the measurability of various components of the Riemann tensor and the need for an invariant formulation of the results of particular experiments. Bergmann also commented on the maximum radiation that could be expected from double star systems. He felt that solutions of the Einstein equations for the radiation problem are far from complete, so that if a measurement could be made it would have theoretical significance.

Robertson asked for general comments before concluding the conference. An apparently new experiment was proposed quite tentatively by Nordsieck. This would consist of sending a very precisely periodic source of radio signals into the sun; analysis of the record of these signals, together with knowledge of the orbit, might make it possible to measure components of the metric tensor that are known only imperfectly.

In closing, Robertson summarized those parts of the conference that are of greatest interest to NASA. There was general agreement with his conclusion that some or all of the three types of gyroscope precession experiments (superconductive, electric, slaved satellite) are promising enough to warrant encouragement by NASA. Cosmological experiments should also be kept in mind as they develop. Roman and Jones expressed appreciation on behalf of NASA for the participation of those present. NASA would like to supply vehicles that can be used for significant scientific experiments, hopes for further feasibility studies, and, eventually, for definite proposals.

To this reviewer, the conference demonstrated the value of a short meeting of a small number of specialists to discuss a closely related group of topics, that could lead to still another fruitful union of science and technology.

L. I. Schiff
Stanford University

CONFERENCE ON EXPERIMENTAL TESTS OF
THEORIES OF RELATIVITY

July 20-21, 1961

Prof. J. Weber, University of Maryland
Prof. R. V. Pound, Harvard University
Prof. R. H. Dicke, Princeton University
Prof. L. I. Schiff, Stanford University
Dr. J. Siry, NASA, Goddard Space Flight Center
Mr. J. Mitchell, NASA Headquarters
Prof. C. W. Sherwin, Aerospace Corporation
Prof. A. H. Taub, University of Illinois
Prof. Peter G. Bergmann, Syracuse University
Prof. Otto H. L. Heckmann, University of California and Hamburg, Germany
Prof. John G. King, Massachusetts Institute of Technology
Prof. B. L. White, University of British Columbia
Prof. Bryce DeWitt, University of North Carolina
Dr. A. Hochstim, Convair
Dr. Edward R. Dyer, Jr., National Academy of Sciences
Mr. David Adamson, NASA, Langley Research Center
Dr. Nancy G. Roman, NASA, Satellite and Sounding Rocket Program
Prof. William A. Fowler, California Institute of Technology
Prof. L. H. Thomas, Columbia University
Prof. H. P. Robertson, California Institute of Technology
Dr. Daniel DeBra, Stanford University
Mr. Ben O. Lange, Stanford University
Prof. Felix Bloch, Stanford University
Prof. W. A. Little, Stanford University
Prof. H. E. Rorschach, Rice University
Mr. M. Bol, Stanford University
Prof. R. H. Cannon, Stanford University
Dr. B. Bloch, Princeton University
Prof. Sidney Liebes, Princeton University
Mr. G. E. Hahne, NASA, Ames Research Center
Prof. A. T. Nordsieck, University of Illinois and G. M., Santa Barbara, Calif.
Mrs. G. R. Caughlan, California Institute of Technology
Prof. J. F. Streib, University of Washington
Dr. Hong-Yee Chiu, Institute for Space Studies and Dept. of Physics, Yale Univ.
Mr. R. T. Jones, NASA, Ames Research Center
Mrs. Helen Drew, NASA, Ames Research Center
Dr. Thomas E. Phipps, U. S. Naval Ordnance Test Station, China Lake

Professor H. P. Robertson, Chairman

Morning Session, July 20, 1961

CHAIRMAN: Ladies and Gentlemen, we want this to be a most informal conference. It is too bad we have no big table around which we can all sit. It will be extremely informal and to emphasize that we have purposely not started exactly on time. There is a so-called program or agenda and I consider this not to have been written down in letters of stone. You can change it around as appropriate. Now, as for the purpose and scope of the conference (I think you are aware of that), we wanted to get together, at the request of NASA, people who are interested in seeing what could be done in the kind of physics associated with the theory of relativity. I am being careful not to say the theory of relativity as such because this is one of the things which may be questioned, but rather the kind of experiment which would be of interest in the field and of the type which is being made possible by the newer developments in rocketry, satellitery, and planetry (or whatever the name for that is). We want therefore, as a number of people interested in this general field of physics, just to talk about proposed experiments and to advise NASA what we think the value of those proposed experiments would be.

I think it might save some time and misunderstanding if I start out just by stating in neutral fashion the situation with respect to the general theory of relativity. I am not excluding, by any means, the special theory of relativity, but I think that experiments designed to test the theory of special relativity can most advantageously be performed on the surface of the earth. They do not involve the kind of resources which NASA could provide. The general theory of relativity, on the other hand, or any theory of gravitation, will presumably be the kind of theory in which we may make use of the NASA facilities. Of course there will be certain effects in the experiments we are talking about which are attributable to special relativity. But unless someone wants to make a point of it I would think that, in general, we are best advised simply to take care of the special relativistic effects in a theoretical way so as to cut them out from the experiment. I am perfectly willing to argue about that point. It may be that some of you have ideas on special relativity which cannot be best tested on the surface of the earth; as a matter of fact, I can see a couple of possibilities myself.

Now I'll talk about the general relativity theory of gravitation in spite of the fact we all know that it isn't about relativity anyway but is a theory of gravitation. It is a definite one, and it is one which has been completely formulated and the consequences drawn, so it gives at least a good point to jump from. Therefore I will write down here on the board certain consequences of the Einstein theory of gravitation which may be involved in our discussions. And I will do that in terms, first of all, of the static model (a Schwarzschild-like model). This is done just in order to get our nomenclature agreed upon. As several of you have done in papers we shall try to isolate the terms responsible for certain effects. It has been found convenient to write the spherically symmetrical line element in some such form as follows. This is the so-called isotropic form of the Schwarzschild metric.

$$ds^2 = \left(1 - \frac{2\alpha m}{r} + \frac{2\beta m^2}{r^2} + \cdot \cdot \cdot\right) dt^2$$

$$- \frac{1}{c^2}\left(1 + \frac{\gamma m}{r} + \cdot \cdot \cdot\right)(dx^2 + dy^2 + dz^2)$$

In the Einstein theory of gravitation $\alpha = \beta = \gamma = +1$.

For planetary motion (but not for problems involving the deflection of light), it is convenient to write the metric in the following form

$$ds^2 = \left(1 - \frac{2\alpha m}{r} + \frac{2\beta m^2}{r^2}\right) dt^2 - \frac{1}{c^2}\left(1 + \frac{2\gamma m}{r}\right)\left[\left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2\right] dt^2$$

$$= \left(1 - \frac{v^2}{c^2} - \frac{2\alpha m}{r}\right) dt^2 + \frac{2m}{r}\left(\frac{\beta m}{r} - \frac{\gamma v^2}{c^2}\right) dt^2$$

where

$$v^2 = \left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2$$

The first term on the left is the largest and is what I would call the classical term, responsible for the Newtonian theory of mechanics. The second term on the right is a higher order term which we may want to take into account. The part proportional to β is about as large as the part proportional to γ. The next order terms are so very minute compared with terms like these, which are responsible for the specific relativistic effects, that I doubt very much that you will have any occasion to call on them. Now the results are these: First of all we have $\alpha = +1$, since with this value we get the Newtonian approximation. This value also comes from the mass red shift. The possible experiments or observations, which can be made or which have been made on the red shift will bear on this term. There are two justifications for $\alpha = 1$: first, the Newtonian approximation and then, independently, observations or experiments involving the mass red shift in various terrestrial, near-terrestrial, or stellar forms. The second effect is one involving the deflection of light in passing the rim of the sun. Now the prediction here is as follows: The bending of a beam of light is measured by $(\alpha + 2\gamma)/2$ times the distance (GM/C^2) associated with the mass of the sun divided by the radius of the sun. What I am interested in is this number, which for the Einstein theory is equal to +4. Now we will hear, and of course a good many of us know, that there are other possible ways of testing this number gamma. One of them I think we will hear about from Dr. Schiff tomorrow perhaps. The third effect is that of the motion of the perihelion of a planet, and the number involved there is a quadratic function of these coefficients. I have not put $\alpha = 1$ in this formula, although I am sure all of us are reasonably convinced that it is 1, excepting perhaps Professor Dicke. I think it's 1 at the moment, or pretty nearly equal 1; however, I am not going to put it equal to 1 because I would like to see what effects are attributable to second-order influences of first-order terms. The advance in the perihelion of Mercury is measured by $2\alpha(\alpha + \gamma) - \beta$. This is multiplied into the mass of the sun divided by the semilatus rectum of the planet. So β, which is a second-order term, appears here linearly, but α, you will notice, appears always in the quadratic terms. I think it is of some interest to keep α in that way just for that reason. You can really see that it is a second-order effect though it would seem to come from the first-order term. And here, of course, in the case of the Einstein theory this number is 3. Perihelion motion is associated principally with Mercury. These are the three classical effects with which you are all acquainted. And as I say, I am putting this down so that we can have a common background to talk from. It is my own feeling that measurement of the deflection of light is the weakest of these three classical experiments. I personally don't have very much doubt of it, but the observational evidence for it does seem to me to be weaker than for the others. The theoretical value is 1.75 seconds of arc which is the net value obtained by Trumpler's analysis of the eclipse results to within their probable error. I think the main criticism which has been made of this is that of Finlay-Freundlich who gets a value which is, however, by no means the Newtonian value, the Newtonian value being 0.87 second of arc; I think Finlay-Freundlich gets something like 2.25 seconds of arc ±0.18.

There is one more effect which I hope will come up during this discussion and which does not refer to the form of the metric I have written. I am referring to the work which was started by Lense and Thirring* concerning the Mach hypothesis. For that one takes into account motion of matter, in particular, for example, the rotation of a shell or of a planet, and then examines the effect of that one the inertial frame, which is outside. That effect goes somewhat beyond the scope of this particular Schwarzchild model and is no longer a static model. Still another effect, which is however bound up immediately with what I already have on the board, is the so-called geodetic effect. Because of the curvature of space the inertial frame which is carried by the laboratory or a satellite does not as a matter of fact return to its original orientation after it has made one revolution in space. This geodetic effect is one which we will be talking about here. The geodetic effect is one for which there is no direct confirmation so far as I am aware, and it gives rise to the following: (writes equation) It gives rise to a precession of a free gyroscope in the amount $(\alpha+2\gamma/2)(m/r)$, where m is the mass of the earth and r is the radius of the satellite orbit. This, therefore, in the Einstein theory would be the number $3/2$. As far as I know there is no direct confirmation to that; however, it is the largest of the effects predicted in taking into account corrections of motions of the moon due to the general theory of relativity. And in the case of the motion of the moon, this effect amounts to 1.94 seconds of arc per century. This value, I am informed by my astronomical colleagues, is just a little beyond the present attainable accuracy. I believe Prof. Thomas could tell us more about that. I believe with the programs now under way one does hope to have that figure for the precession accurate to within 1 second of arc per century; perhaps you can say something later about this.

PROF. THOMAS: There are two things necessary for this, improvement in the observations and improvement in the theory, and both are going ahead. It is believed that the next time the theory is compared to the experiment it should be possible to get an estimate of this effect.

CHAIRMAN: Now I think that that effect is an extremely interesting one, at least from my standpoint it is, because it involves γ alone. And from my point of view γ is the least well determined. Of course, I am still talking within the frame of the

*Phys. ZS. 19, 156, 1918.

theory of relativity. If we want to talk about alternative theories, of course, this is an entirely different situation. Well finally just let me write down, although it doesn't quite belong in this category, the Lense-Thirring effect. In that case we will have to introduce an additional term to the line element of the form $2g_{0\alpha}\, dtdx^{\alpha}$. This $g_{0\alpha}$ is usually for the planetary applications of second order. In some situations however the Mach effect is kind of a first-order term. For example, if I took a hollow shell and determined the field inside the hollow shell, and if m is the mass of the shell, and a the radius of the shell, then this term will turn out to be proportional to $4/3\ m\omega/a$, linear in ω. My units are implied, of course, by the things which I have here. Perhaps we can just take that as a background. If there are remarks anyone would like to make on that now, let's have them.

QUESTION: Would you say again what the geodetic effect was?

ANSWER: The geodetic effect is this. Let's take the case of the earth going around the sun and consider it as the free gyroscope. (see figure) Then the world line of the sun, which is the spatial reference in this case will be straight and then the earth we'll draw around here in a helix. Now the local inertial framework is defined along the geodesic and, of course as I go along, this framework is propagated by parallel displacement so that it remains tangent to the geodesic. Let me bring this up here so the system goes around and comes back to the initial spatial position. The unit vector is still along the tangent. However, I will have, in addition, three spatial vectors and as I propagate them by parallel displacement along this geodesic they will return to a certain spatial position. Now that spatial position will, in fact, differ by a small amount in angular measure from the initial spatial position. The amount is given by this quantity. This is what I am referring to as the geodetic effect.

FROM AUDIENCE: You won't determine it from the measurements along the geodesic itself; you will have to orient the measurement with respect to the sun?

CHAIRMAN: Yes.

BERGMAN: About 2 years ago Pirani and Tiry estimated the order of magnitude of the effects 4 and 5* relative to each other. He was the first to consider doing experiments with gyro-stabilized satellites. At that time he did a very rough calculation. It seems the ratio between effects 4 and 5 is the ratio between the period of the satellite, that's for the geodetic effect, versus 24 hours,

*Geodetic effect and Lense-Thirring effect, respectively.

for the Lense-Thirring effect which depends upon the rotation of the earth itself, so that this ratio is about 1 order of magnitude, no more. You would, therefore, have to estimate both of these effects and measure them together, and perhaps separate them clearly by the difference between the meridian and the equatorial effect.

CHAIRMAN: This would depend on the distance, the height of the satellite.

PROF. L. I. SCHIFF: The calculations and detailed results have been published and I will talk about them tomorrow. I'll describe the Lense-Thirring effect along with the geodetic effect. The secular changes have been calculated and also the detailed rate of change in the angular momentum. There is an additional factor here which is the ratio of the radius of gyration of the earth to the radius of the orbit, and this is in the wrong direction since it makes the Lense-Thirring effect smaller.

CHAIRMAN: We don't want at this moment to get the results of the whole conference so I will apologize for leading into a controversial field. I was giving my concept of the situation before the recent clashes. I think it would be appropriate to start out as the program suggests with Dr. Pound and his statement on the measurement of the gravitational red shift. This deals with α, which is the first-order effect.

PROF. POUND: Well, the informality was extended to the point that this was my first observation of the program and therefore I am not quite clear just what form of talk I should give about this particular thing. I have talked about this subject for over a year now in one phase or another. I presume most everyone has heard it in one form or another over that time, so maybe I should just try to say a few special things without trying to describe our whole operation.

CHAIRMAN: Might I suggest that one of the things which NASA wants us to do is to advise them on future experiments. Now we all know there have been some proposals to measure this effect from the satellite. I think we should consider the problem. Do we still think it's desirable to attempt to measure this?

PROF. POUND: Well, first of all, your remark that this mass red shift was one of the demonstrated effects. I have no particular claim to expertise as to the status astronomically as you probably have, but my impression is from review articles, particularly Finlay-Freundlich, for example, that in reality the astronomical evidence was completely inconclusive.

CHAIRMAN: For what?

PROF. POUND: For the mass red shift.

CHAIRMAN: From the sun?

PROF. POUND: From the sun and from B stars and distant nebulae as well; that is, the sun gave the average value over the disc about one third of the effect expected and the distant stars gave an average of over 10 times the effect expected in his particular review.

CHAIRMAN: Maybe we can hear something of this from our astronomers.

PROF. POUND: I would like to hear something like that because actually quoting those numbers and looking at the reviews one still doesn't appreciate just what's happened to get those numbers in print. Does this constitute the measurement of the centroid shift in some line compared to some other line, and really how creditable is this evidence for that number? But at least I have an impression that it's much less well founded than the general public had been led to suspect by the level of the statement that's usually included in a text book, or in Einstein's Second Edition which has an Appendix from the first edition which said that it was gratifying that this effect had been demonstrated. I think it perhaps relevant to bring up the fact that I have often entitled this subject or this experiment "A Measurement of the Apparent Weight of Photons or of Radiation" as distinct from calling it a gravitational red shift. I have been subjected to considerable argument by people who take the view that this is a terribly bad thing to say and that it is pedagogically bad, because they say this isn't really what happens. What happens is that the time scale of the system at one gravitational potential is shifted with respect to the time scale of another, and what you observe is a difference of their clocks, when you communicate the information from the one to the other. Now this is the attitude that is produced when one thinks of how to do an experiment in space using a satellite. One thinks of using an atomic or some other accurate time-keeping device which will integrate its oscillations up to some point and then send a very rapid signal back to be compared with a similar device held at the surface of the earth. Now the question is whether this or the other is the right attitude. I don't pretend to be in a position to argue, but I would like to point out that none of this is really relevant in the prediction or in the systematics of the experiment that I have done, and therefore I prefer to use the concept of apparent weight since everything about the result of the experiments that I have done on earth can be derived with the simple concept that radiation has weight. Or, let's say, if we want to ask for the apparent frequency of the source that radiates to a detector in a region of uniform gravitational field, expressed by the fact that material objects would fall with acceleration g the principle of equivalence then introduces the concept of the box accelerating kinematically at the rate of g. Then all we do is introduce the concept of the first order Doppler effect, which occurs because between the time of emission from the source, during the time of travel, the velocity of the receiver changes an amount of gh. If h is the distance h/c is the time of fall, and g is the acceleration, all that happens is that the velocity of the detector is different by the amount from the velocity of the source when the radiation was emitted and therefore a first order Doppler effect $\Delta v/v$ arises which would be just $\Delta v/c = gh/c^2$. The point I want to make is that this is a simple conclusion and does not involve the special theory of relativity. Nothing about this proposition has introduced the idea that the velocity of light is constant. We are definitely talking about a very small Δv and whether I use a constant or not here doesn't matter from the point of view of this experiment. What I want to say is that when you come down to it, the most fundamental statement about the experiment is that it has measured the velocity that is necessary to give the detector in order to remove the effect of having put the source above it as this certain distance h. It has only measured that velocity and that is precisely the velocity a material object would have gained in free-fall for the same length of time. So therefore, as far as the experiment is concerned the results are precisely those which would be gained by using the concept of weight. And I contend that this idea is the least pretentious and the least extended conclusion from the experiment itself. So much for that point. Now as far as the experiment is concerned, I think all of you know that it is based on the Mössbauer effect. Utilizing this effect together with a careful technique of slope detections we were able to find an apparent shift of the center frequency of the line with height. The height of our path was about 70 feet. With this height the fractional effect was about 2.25×10^{-15}. The stronger the line the better you can deal with it even if it's not as narrow as some weaker lines. In reality a slope detection device is what one is looking for. The problem is that of finding the transmission through a resonant absorber which changes slightly with a slight displacement of frequency. And we make those changes purposely with velocity devices so as to compare the plus and minus velocities with each other. Other tricks are used for self-calibration of the system so that we don't have to know anything about the actual line width, shape, or depth. Now if we look at this we are dealing with a line whose full width at half intensity is equal to about 10^{-12}. Ideally if everything about the solid state could be controlled the experiment should be good to about 6×10^{-13}, but we haven't come quite to that accuracy. Just seeing the effect in our system constitutes a splitting of the line to the order of 1 part in 400. Now we put in a lot of devices to try to control sources of systematic instability. We have such tings as not just one detector but two detectors assembled right near the source and if things about the source change in principle, that doesn't really matter because we only need to compare the differences seen by the near one and the far one. When we invert, we invert the whole system, keeping the near one and the far one in the same relative position. There is thus a first-order compensation for variations of a systematic nature such as in the modulation wave forms we use. When all is said and done there are certain practical limitations. Let's say if you look at the thing from a theoretical signal to noise ratio point of view, it looks as if the ability to measure this quantity is independent of the height of the path. Statistically the ability to say that two counting rates are different from one another is a function of how many counts one has and as one increases the path length the number of counts goes down as the effect goes up. These two are exactly equal and opposite and therefore they cancel each other. Thus in principle the ability to measure the thing to any accuracy is independent of the height. This, of course, isn't true in practice because, for example, suppose one takes two sets of scalers and absorbs the total number of counts on them. Each scaler has been opened to radiation for exactly the same length of time but they each have had random counts coming into them during those times and the question is, how accurately can you get the two to count equally over a long time? In principle, if we try to do the experiment on a table top we would have to assess counting rate differences of the order of one part in 10^7. In practice, the number which we get out of our machinery, corresponding to the whole shift, amounts to a counting rate difference on a pair of scalers of the order of a tenth of a percent. So that when you start to talk about measuring that effect to 1 percent, for example, you are looking at the requirement of having two sets of scalars that count exactly the same to 1 part in 10^5, and what you have to do is to know that they can do that. Now in order to know that they can do that, you have to put as much energy into looking at random counts without signals as you do into looking at random counts with signals. And you have to have this done under all the same conditions as the actual experiment, so essentially there is the way to double the length of time doing the experiment, or actually more than double it because you would like not to double the statistical error in the end. It is evident that there are uncertainties one can't completely assess. Of course, as most people know, about mid-course of the experiment we discovered that the main instability we were suffering from was the effective temperature of the source. We were rather reluctant to really admit to this until it was forced on us by the result of attempts to find the cause for the instabilities in the effect of magnetic fields and other things. We finally discovered that the current in the coils was heating the thing, and this led to the change of temperature. When we put heaters instead of magnet coils they also produced the effect. It was realized in fact that this is precisely what should happen when you consider the time dilatation of the coordinate system of the particles doing the emission or detection. This is simply the effect of their having different average values of $v^2/2c^2$. Once we recognized that, we found that we had to control the temperature. Either we have to know exactly the correction to make for a given temperature difference, or we have to know that they are at the same temperature. We chose the former as the most expedient at the time and in the future we shall choose the latter. Although we will choose a combination, we shall try to keep the difference small so that we don't have to know the correction coefficient too accurately, and at the same time we shall try to measure this coefficient more accurately. During the operation we took data to improve the results statistically. We also tried the effect of changing the system from time to time in order to see whether systematic changes resulted. No such changes were found in the independent runs, but the sum of all of our runs led to the value of 0.97 ±0.035, with the statistical error only. This error may be slightly overestimated. The statistical error could be smaller if one recognized the fact that the measurements of the sensitivity of the system and the measurements of the asymmetry are not really independent. We use the same counts and therefore incurred an increase of the statistical error from treating them as if they were independent statistical numbers. Now what about the meaning of this - Do you believe it? Well I would say right now that if somebody had a better experiment which had measured this effect and found that it is as much as 10 percent in error I could still justify that error for our apparatus. In other words, I think that our uncertainties in the knowledge of the temperature difference on the average for all the times of operation, uncertainty in the knowledge of the temperature coefficient correction, certain fluctuations in the data indicating a certain inability of the counters to really count equally, all of these things might lead to an error of 5 or 7 percent as a possible limit of error. In other words, may I put it another way and say I would be surprised if anybody could show that the correct answer was more than 10 percent from the predicted unity here. But so far this is just a number from my statistics. Now where does the experiment go from here, and how does it bear on the space vehicle problem? I think there may be some point in the fact that this is a different experiment in the sense that this result does not really measure frequency directly. Essentially, I think to add this to a satellite experiment with a clock comes down to a test of the special theory again, the applicability of the special theory to the two points, the ground and the satellite. I think it would be a long time and a very expensive proposition to try to better the measurement of the number here by the other means. You should remember that the expenditure on this program with this accuracy requires less than 1 percent of the cost of the fuel for a rocket. If you want to decide which would be the better way to do the experiment, try giving us a staff and the kind of money that would go into rocketry, and I think one could extend this technique by at least one order of magnitude. In fact, I hope to do just this without that kind of money. I think the technique could be extended by possibly two orders of magnitude if it were given. Our experiment was done with the very first gamma rays sought for this particular application. Mössbauer first described the effect and it is known by his name. He made no remark that it was a fractionally precise radiation and when we recognized this fact we looked through the literature for an isotope that has this exclusive property. Three or four people who did this came up with the same two isomers. This one and zinc. Zinc has a narrower line by a factor of a 1000 or so in principle but has so little intensity that it is practically impossible to use and, in fact, has not been shown definitely to have a narrow line, in view of a lot of solid-state effects that come in and give inhomogeneity and broadening for various causes. Certainly one has to look further into the sources of systematic error. One which we have explored just now, and which Dr. Benedict, who is experienced in high pressure physics has helped, is the effect of pressure on the gamma ray energy. This effect has nothing to do with relativity in particular, except perhaps in one way, connected with the fact that the vibration velocity is a function of the Debye-temperature so that when you change the lattice constant by squeezing the solid down you change the vibration amplitude and thereby introduce a modulation of the time dilation effect through this phenomena. Quantitatively it turns out to be a couple of percent as big as the effect of the change of the electron density at the nucleus. This is what is being called the isomeric shift of the gamma ray energy which results from the fact that two nuclear levels have different radii, and therefore the nuclear energy state is slightly changed by the chemical configuration at this time; or, if you have a solid, if you can change the electron density of the nucleus by just squeezing it in, then gamma ray energies change. We have measured this change in iron and find that as far as we can see it's exactly in agreement with the renormalization you will use to find the change in electron density at the nucleus. In other words it's proportional to the inverse of the volume of the nucleus, although, you have to have a coefficient which says how much is due to the electron density in order to know what fraction of change this contributes. And this we take just to be the outside electrons, the ones most strongly affected by the volume of the lattice. And there are data from chemical studies about the difference in frequency of the iron nucleus with or without the 4s electrons. You take that as a coefficient then you put the electron on and squeeze the solid and find that it would change its frequency. We wanted to know this for two reasons: One was that it has to do with the temperature coefficient of frequency which is involved in our corrections of these data, because so far we used the theoretical evaluation on the basis that our experimental evaluation agreed with the theoretical value based only on the time dilation. We first measured $d\nu/dT$ at constant pressure; we now measured $d\nu/d\rho$ at constant temperature. Well, of course, these are connected together by the fact that the volume changes with temperature; if you work that out you contribute something of the order of a 10-percent correction to the temperature coefficient at room temperature. The statistical error in our measurement of temperature slope was on the order of 10 or 15 percent anyway. It was in the low direction, which is the direction this effect would account for. Of course, just for completeness, one would want to be sure that just the atmospheric pressure change didn't produce enough change of frequency of the gamma ray, and until you do an experiment you can't rule it out. This is a few orders of magnitude away. Well, I think this rambling is all I'll say. I don't know if this is what you had in mind.

QUESTION: What do you propose for the future?

ANSWER: Let's say we stopped taking data when we got to about this level, about last October, and began to explore ways and means to improve the sensitivity and rate of collection of data, the stability against the systematic error, and also the possibility of a longer base line for the experiment. The first period we lost a couple of months in trying to make stainless steel behave as a source or absorber on the basis of other people's claims that it had a narrow line width. The other people were wrong on the basis of our demonstrating otherwise. That was pretty much a loss of time. It turns out still that iron, excepting for the hyperfine structure that you have to accept in it, is still the best. We still have to use an iron source, and we have to find a better absorber, that is a physically better example than an enriched iron absorber in order to avoid one of the sources of error. That is, in our experiment the source and the absorber happen to be different by about 1 percent. In principle this doesn't matter as long as you reverse ends. However, it would be better from the point of view of an accurate experiment to get rid of most of that displacement because we are best off if we can assume that the line shape is such that it's a linear detector. That is, we want to get the signal directly proportional to frequency displacement, and when we have a large apparent displacement, it may be a fair fraction off the center of the line toward a point beyond which we start getting a second-order term in slope detection. Another thing of course is that the source has a 250 day half life. We withheld getting our second source until we were able to use it, with all the things we wanted to do. One of the things was to build a better temperature control and measurement devices. In this experiment we measured temperature differences by one thermocouple with one junction at the source and the other junction 80 feet away with its center pole on a plate that contained several pieces of beryllium coated with iron acting as a deflector, but you can see that one junction could hardly be claimed to sense the proper mean of temperature over this 16-inch-diameter device. We have been forced to recognize that for this particular situation, proportional counters have an advantage over the scintillation detectors, because they are available with smaller thickness. We are able by this means to reduce the counting of background gammas (that are actually 60 times more numerous), so that we can actually raise the useful counting rate in the window; and by this means we can, with a single channel of electronics, still count about 30 times faster that we were doing in the main experiment, and we can tolerate a bigger source strength that way. With a bigger area detector, with a larger source, about four times bigger, and with this proportional counter we expect to be able to do an experiment equivalent to this about every day so that essentially we are beginning to be in the position that we can test for systematic errors without consuming the half life of the source. I would expect by these means even using our present site we can probably reduce the overall error by something like an order of magnitude or to something like a half percent. Finally, obviously the correct step is to look for a longer base line and then our present control of systematic errors would allow improved accuracy. One difficulty of that is I don't particularly like working in mining shafts and I doubt that we would be too successful in maintaining our electronics which is fairly hard to maintain even in the laboratory. That's one of the places where a larger scale investment would make quite a difference. I'm not sure I would be the one to manage this thing. The other possibility is a building. We aren't sure about what the effect of vibrations in the building are but I am sure thermal expansions and contractions can be taken care of but the vibrations which get one outside the velocity tolerance of the linear region slope detection are liable to give you some troubles. But Boston isn't very well endowed with tall buildings, the tallest one I have investigated has an elevator in all 18 of its shafts and all running at the moment, and I haven't approached the right level to see if they would take one out, or stop them. They have about 350 feet vertical height, say, of the John Hancock building, and then there is a possibility of a New York building. But one would certainly have to investigate the vibrational aspect.

PROF. SCHIFF: As you said once, if you increase the height, the statistical errors increase so that you don't gain anything.

ANSWER: Yes, I said that today, too. That the statistics and the signals have to compensate so that in principle you don't gain anything by changing height, but from the practical point of view, for example, where we can't even make two counters count equally to one part in 10^6 where sampling different time samples alternately from the same source, you're led to believe that you would be better off to make the effect one part 10^3 than one part 10^6, even though the principles of statistical fluctuations would say this isn't necessary. But that's just not practice historically.

PROF. FOWLER: Is there any possibility in trying to go to a higher accuracy that difficulties will arise from the standpoint of a fundamental understanding of the Mössbauer effect itself?

ANSWER: The recoil phenomena is not fundamental in itself, it's just that the radiation is stable. It's a question of understanding. I think people in naive times looked for lifetimes of several seconds and such things for getting really narrow lines, but at least we knew that nuclear spin interactions will dominate the line with a level not much below this. Because it will be at least a term of the line width derived from the ordinary solid-state spin-spin interactions which is just like what we are quite used to in nuclear magnitudes and that is of the order of several kilocycles. There is no use going to line widths that are smaller than that. In particular in solids that the electroquadrupole inhomogeneity leads to quite enormous spreads of line widths in terms of radio spectroscopy. This is why stainless steel is no good in this iron situation because it is an alloy and the local surrounding of an iron is only 50 percent iron, 20 percent cobalt, and 20 percent nickel or chromium. These are not enough alike chemically to lead to zero field gradient; instead there is a small field gradient and that produces line width effects. Now you see that in radio frequency terms a line width of one megacycle is fairly large compared to the typical solid state line of nuclear magnitude. To go much beyond that line width you start at 10 kilocycles and get right into the domain of the nuclear magnitude of line widths and I think that is the end from the point of view of narrowness. Now there are particular tricks to minimize this but I think the quadrupole one will always do you in because one or the other state will have more than it's going to have. And as soon as that's true you have this homogeneity problem.

PROF. FOWLER: Well the thing we seem to be worrying about now is the fact that perhaps another time is involved in the basic emission process of the photon, the time for the photon to be emitted, not the lifetime of the state.

ANSWER: But those are the same. What's the difference? This whole lore is that of the interaction between levels and radiation. I think this subject would be better looked at from the point of view of nuclear magnitudes. We have done alot of thinking about that kind of thing. I don't think there is much there and, even so, I think all that he is worried about there is the actual discrepancy between the measured line width and the lifetime width. Everything that we measure in our system is the velocity measurement. We don't care what the line width is or what the depth of the absorption is.

PROF. WEBER: I think the lack of understanding is not dependent on the gravitational field.

PROF. FOWLER: Of course we wouldn't do the experiment unless it were to some extent and that's sort of what we hope to find out.

PROF. SCHIFF: To get back to Robertson, do you see any possibility of extending this to things that are of interest to NASA? Satellites or rockets?

PROF. FOWLER: You mean the Mossbauer experiment?

PROF. SCHIFF: This kind of experiment?

PROF. FOWLER: I will say to those people who take the Mossbauer effect as the way of measuring gravitational gradient, I might point out that in one stage here where I conversed with you I suggested we might find a 10 percent error, which of course turns out to be a zero shift in the thermocouple. I'd say that this is the most difficult to calibrate thermocouple ever made, and there was also an effect due to the height of the building, I made a mistake in scaling the drawing which was the basis of my statement of how high the building was. That basis is inherent in the result. And in that sense, I am afraid that people would like to pick up the phenomena and use it because of its particular glamor or something.

There are much better ways to measure this. For example, there was a question whether the line is split by the inertial anisotropy, which is much better done by a different measurement because that is not a fractional thing. For measuring an absolute interaction, then, nuclear magnitude can't be beat.

MR. JONES: Prof. Pound, you have compared your experimental result with the first-order Doppler shift that would arise in an equivalent kinematic acceleration. Of course, this immediately suggests that you just turn the apparatus on its side and apply the kinematic acceleration and compare the results.

PROF. POUND: Well we applied the velocity. In fact, in the operation of the experiment, we apply the velocity, or actually range velocity, and interpolate it to find the velocity of the source which would be running away from the detector which would compensate for the change of the velocity if the radiation would gain in falling, if you will excuse the correction.

MR. JONES: So then one could isolate that.

PROF. POUND: It's much harder to find a 75-foot long horizontal helium bag than a vertical one.

MR. JONES: There are probably rocket sled tracks in existence that are longer than these buildings.

PROF. POUND: You are not just trying to find a field free region. Yes, there is a question of whether an accelerated frame does the same thing. Well, surely that's been well demonstrated for years in various aspects of the demonstration in the rotation time case. That's just the old-fashioned quadratic Doppler effect that accounts for the mass change of cyclotrons and which was observed, in fact, by Cranshaw and others by the Mossbauer effect directly, but which is also inherent in our temperature coefficient. Actually, in the calculation you only make use of the acceleration with the change of velocity it gives. So if you just put in the velocity steadily, which is the way you do it (by putting it into rotation), why then you get the effects straightforwardly.

PROF. WEBER: I wonder if Prof. Ramsey has calculated how precisely he could do this experiment if he put his atomic hydrogen clock in a space vehicle.

PROF. POUND: Well, he has no evidence yet that his atomic hydrogen clock is sufficiently stable. As a possibility, he says that he is shooting for 10^{-13} fractional stability. Now to me that means the fractional stability that you could get after you have done all the integrations that we have done here to be compared to our present 5.10^{-7}. Thus you would have to have a height of the order of 10^{-4} times greater than ours in order to get the same slope. Now it's true that certain theoretical limits of his thing would give a correlation stability which means a short term stability of 10^{-15}. He has for example temperature coefficient which is at the moment 60 times bigger than ours. And the reason for that is that this hydrogen whose rest mass is 1 instead of iron whose rest mass is 60.

PROF. WEBER: My principal reason for asking this was I was just wondering how far the wildest extrapolation in present atomic clock techniques would be enough to get the next order of correction, the general relativity correction, to this red shift formula in the satellite experiment. This would require long term stability at one part to the 19th.

CHAIRMAN: In this connection, Prof. King, I wanted to call on you later in response to this present suggestion. Can we postpone this until Prof. King can talk about the atomic clock?

PROF. POUND: The next correction to this is to just use the correct Doppler effect.

AUDIENCE: But that gives a different result than the general relativity result.

PROF. POUND: But that comes into the level of this squared in particular from the level of 10^{-15} to 10^{-30}.

CHAIRMAN: By the way this hasn't really become a conference yet. I would like to find out if there is anyone here who has a plan for doing a laboratory experiment as opposed to a satellite experiment of this nature. But first, suppose I call the roll, I have here a list of people who we hoped would be present. It is not complete and I also know there are some who are not here. I would like to have you identify yourselves. I forgot to identify myself, my name's Robertson. (The chairman calls on members of the conference individually to stand up and introduce themselves.)

RECESS: 10:30 a.m. Thursday.

CHAIRMAN: Dr. King has found an article in the IRE which he wants to look at before talking. Before Dr. King goes on with the atomic clock and the satellite problem, I would like to repeat the question that we had before. Do any of you plan, or do you know of a plan to perform an experiment in the laboratory equivalent to Pound's?

QUESTION: To perform the experiment in the laboratory?

CHAIRMAN: For instance what are the people of Harwell doing?

PROF. POUND: Well my understanding was that last summer they had no hope of getting anywhere near our statistical error so they have quit. They had not recognized the possibility of any inherent shift in the line; their results were useless without that and the Doppler intensity effect.

PROF. TAUB: I think (?) at one time had plans for this but I think he too has given up.

PROF. DICKE: Someone in Chicago wrote me at one time, I've forgotten who it was now, saying that he was thinking about doing it.

PROF. TAUB: There are some people at Argonne who are thinking of doing this.

CHAIRMAN: One topic that was mentioned is the question of that so-called mass red shift on the sun and on the stars. I think the situation with respect to the sun is really confused because there are so many effects which are not thoroughly understood. Recently in the Monthly Notices there were two different papers in which corrections and so-called limb effects and the constants used by the two different observers were exactly the same even though they had opposite signs. The history of this matter begins in the early 20's. Adams, at Mt. Wilson, thought he had detected the red shift on the companion of Sirius in an amount which was consistent with what was then thought about the nature of white dwarfs. After many long years this result has been written off to a very considerable degree. Work done by Daniel Popper on the aerodynamic side indicates a red shift quite consistent with the structure of white dwarfs. Heckman, could you say something concerning this?

PROF. HECKMAN: The complications of the sun are understood in principle. These arise in the small streams going downward and upward in the solar atmosphere which appear as granulation of the sun. The upward streams are somewhat stronger and brighter and therefore able to produce a violet shift. If you subtract the amount that corresponds to the upward motion, the rest corresponds to the Einstein law, but I would not say it is of 10 percent precision.

PROF. POUND: Do you have an independent method of assessing this number you subtract?

PROF. HECKMAN: No. You just make the difference.

PROF. POUND: Then do you really know that it rises? I had assumed that it rises on curve looking from the mean value, but I can't say.

PROF. HECKMAN: I'm not able to talk about these details.

PROF. POUND: My impression in other words is that if you can justify that answer, it is good evidence of the appearance, but I couldn't say it proves the red shift.

PROF. HECKMAN: No, I wouldn't say it proves the red shift. You need more than just the currents, and if you take Einstein's theory it would give you the rest.

PROF. BERGMANN: I would like to add to Dr. Heckmann's lack of expertness by my own lack of expertness. The granulations on the sun were confirmed by M. Schwarzchild from photographs of the sun which gave the theoretically conjectured size of the convective source and lent some reasonable substance to the theoretical estimates of what order of magnitude one subtracts or adds.

PROF. DICKE: I would like to add a word. Another problem, of course, is that when you go out to the limb, you might well expect to get the proper value, but it is not clear how you get the right shift at the limb. I might also say that one of our students is building a new kind of spectrometer for the specific purpose of looking at the sun. We have the feeling that much of the trouble is connected with nonlinearities in the photographic plates; with the distorted lines you get nonlinear effects. It is very difficult to get a proper measure of the center of the line and it's possible that measurements made with this might help unscramble it.

CHAIRMAN: Well with respect to the white dwarfs, Finlay-Freundlich 10 years ago questioned the result and subsequent to that I had a talk with D. M. Popper who feels that his work is correct and gives a good confirmation. He thinks, in particular, he has found the shift in Sirius B. I said to him, from my standpoint, in view of the work of Pound and other theoretical predictions based on it, I would consider that the red shift would be a valuable number for people dealing with theories of stellar structures, and I got the usual vague concurrence that any theoretical fellow gets from an observational fellow. Willie, do you know anything about this?

PROF. FOWLER: No, but there are many complications in connection with white dwarf results, too.

CHAIRMAN: In the interpretation of the shift?

PROF. FOWLER: Yes.

PROF. POUND: I'm not an astronomer but I have the general impression of what one means by having proved something, and it is the equivalent of putting down what I regard as our statistical systematic errors. I would like to see somebody do that in an astronomical statement.

CHAIRMAN: The only recent paper I know of is that of Popper, I can't quote what kind of accuracy he gives.

PROF. POUND: But he does give his accuracy. Does he also put in a number for the uncertainty of the assumption that he has put in to estimate the background velocity?

CHAIRMAN: A good question. The number which he needs is an estimate of the mass of the star.

PROF. DICKE: In connection with the white dwarfs did he know the orbit?

CHAIRMAN: I thought he knew the orbit. 40 Eridani B. Well that's something to work at.

DR. ROMAN: I should think that star would have a good orbit; I don't remember what the lines are. Sirius B has extremely broad lines. Now there are two types of white dwarfs; some have extremely broad lines and some have much sharper lines and I don't know which class 40 Eridani is in.

AUDIENCE: I think 40 Eridani has very broad lines and narrow cores in the lines.

CHAIRMAN: Dr. King, I think it would be well if you told us first just what your project was.

DR. KING: First in 1953 Professor Zacharias started building an atomic clock which was to use falling atoms. The idea was to try to get a one-cycle line width at 1000 megacycles, or one part in 10^{10}, which one might split to an amount depending on how many slow atoms there were, and what the signal to noise ratio would be. The notion was that this clock could be carried up a mountain and one could perhaps see the red shift. The experiment was planned when there were no satellites and no Mössbauer effect, and a lot of discussion centered on whether the experiment should be done by having two such clocks, one of which drives up the mountain in a truck and is brought back down again or whether one should just count cycles and record them on a piece of paper and carry this paper down or whether one should transmit up and down the mountain. All of these things I think are reasonably straightened out now. The effect is gh/c^2, or about a part in 10^{13} per mile. With the advent of satellites one will now have the possibility of using thousands of miles and getting very sizeable effects and so the experiment then becomes one of putting in orbit a relatively crude atomic clock, since an atomic beam tube of considerable precision would have been fairly bulky and subject to accelerations, and one would have to make sure it was working correctly. Then we decided, however, that perhaps the effect could be done with crystals. If one could compare rapidly, that is, have a satellite in an orbit, and a ground station, and transmit and receive while the satellite passed overhead and try and observe the difference in frequency right then and there, the stability requirement would then be a great deal less. This would be in contrast to a method where one would just let the clock run at a different frequency. After all, the same apparatus could do both. Proceedings of the IRE, vol. 48, pp. 758-760, contains the distillation of our thinking on this quick comparison experiment. The notion is very simple. One has a satellite going around in orbit (draws on board) and here is the earth. One transmits up at the frequency f which the satellite receives modified to f' by the Doppler effect and by the gravitational effect, and it mixes that with a frequency 2f and transmits back down to the receiver 2f-f', which is then compared with f. With an arrangement like this you can see that all the first-order Doppler effects or anything that depends on the relative direction of motion, cancel out very nicely. Only the second-order Doppler effect and the gravitational red shift remain. As I remember it the various pieces for this experiment were being assembled about 2 years ago. They involve using a crystal oscillator because now one can get away from atomic clocks since the comparison is made more or less instantaneously and of course can be made repeatedly at different stations. We did experiments with crystals over hundred-second intervals and found that the stability would be about a part in 10^{11}, over about a hundred seconds. These are just ordinary crystals; since then we have done a great deal more work in trying to improve our crystal oscillators. Secondly, transmitting path variations: some of this, of course, if removed by a method of comparison (writes equations) and that which isn't we have observed by setting up some links to and fro from the John Hancock Building and the conclusion was that this wasn't going to be serious as an experiment. I can't back it up with numbers. I think there were gentlemen involved down there building nice minaturized little things for a satellite and that's about where the project ended. Now what is left of this and is still going on? We are still worrying about crystal oscillators because we still want to drive atomic clocks and we want to build up a series of frequency standards suitable to drive more and more accurate ones. Of course, the national atomic clock is typical of electric engineering practice, to show the power of their feedback techniques and the fact that you can pick out the right signal if you have suitable filters and things, and phase locks coherent detectors, and things of this sort. Therefore if you can do all that why shouldn't I let all the other noisy signals in the world float around in the device. Unfortunately we are building a coddled clock, and I think we will be able to get much improvement. But this now diverges from the satellite and red shift experiment. Lastly, of course, the big clock and the falling atom failure, we finally established quite conclusively, was simply due to the fact that there were not any slow atoms in the beam. The Maxwell velocity distribution does fall off, some distance away. That is because we were able to raise the pressure from 10^{-10} to 10^{-9} in the apparatus and found that the beam went down markedly. This was observed on one side by looking at the slow atoms with a shutter and a time-of-flight device. As Zacharias said "how else would you have found out that there were no slow atoms?" You would have had to build almost as complicated an apparatus with almost these vacuums.

AUDIENCE: What's the explanation for no slow atoms?

DR. KING: They are scattered. The cross sections are much bigger for cesium-cesium scattering than was thought before the gas bottle experiments. Of course, we selected them with magnets and devices of this sort as you may recall. The general upshot of all of this is that having built one big apparatus that used 10 man-years, we were hesitant to build a 300-foot long apparatus with a cesium beam, which is probably the way to get a very good frequency standard. But it will be some time, I believe, before one has faith that the paraffin doesn't become hydrogen soaked, or something of this sort, over a long period. This has to be established by experiment. We would like to have some free atoms hanging around in some regions, but there are some difficulties in this. Then having done that, we would like to build a series of clocks with different atoms and different molecules and things of that sort. So you see then that this project which was relevant to our interests today has now drifted off into other things and that's just about the status at the moment.

CHAIRMAN: You have then no intention of going ahead in the future to ride on some satellite?

DR. KING: Well I haven't any idea. I think the general feeling is just as Prof. Pound said. This is a big operation and it should be done I feel, but obviously this is a different experiment and it represents plugging other chinks, so to speak. But there are other things to be done. If we had nothing else to do I would say well let's go ahead and do this. I think the situation may be compared to the time when it was important to show that gamma rays went along with the speed of light. But now how much should we try to do a precision measurement of the speed of gamma rays? I think the answer is that unless there is a clear-cut feeling that there is a reason to do it that makes it different from the Pound experiment or if we could get vastly more precision, and I might mention that the general conclusion was that one could tie the effect down to about 10 percent, so you are not winning at all relative to Pound, unless we have some bright idea of how to do it simply, so that it looks costless, we are just not going to do it anymore. I think that is the general conclusion.

CHAIRMAN: Dr. Roman, Hughes Aircraft was also interested in this type of experiment, can you say something about that?

DR. ROMAN: Well the status of that is very much the same as the status of the MIT experiment and also the status of the one at the one at the Bureau of Standards. We started out by funding three groups and three approaches on how to do this. MIT was originally interested in a flying atomic clock; they then went over to this crystal transmitting approach as being simpler. Hughes Aircraft is interested in doing this with an ammonium maser; again the object was to get a good clock for use in a satellite. Their primary approach has been to use the ammonium 15 maser and they have finally produced one in the laboratory which is close to working and are continuing it on their own funds because they did want to get a working laboratory version; but there really has been very little attempt to miniaturize it and there is some question as to whether it can be. At the Bureau of Standards, Bender was working with the rubidium vapor frequency standards. He did produce or has produced in the laboratory standards which seem to perform quite respectably. He is also continuing work on them, although we are no longer funding him, and his apparatus is appreciably more miniaturized than the Hughes apparatus; it is not completely a flight model but it's beginning to approach a flight model. That is the status of the three approaches at the present time.

DR. KING: Those other two groups differ from us then in that they intend to do it.

DR. ROMAN: Well, I don't think Hughes is actually going into a flight model. They simply asked if we would mind if they waited to submit their final report until after they got a bench model going. Bureau of Standards is in much the same situation, except their bench apparatus is a lot closer to flight form than Hughes' bench apparatus.

AUDIENCE: Do you happen to remember any numbers? One part in 10^{11} for Hughes or Bureau of Standards?

DR. ROMAN: No, I don't. My memory is that Bureau of Standards is getting something on the order of a few parts to 10^{11} - I'm not sure of that. It's a figure I could look up but I would rather not be quoted until I do. I don't remember any figure at all for Hughes.

CHAIRMAN: Insofar as that term α is concerned, and I don't want to say anything that Dicke will disagree with if you are arguing about the complete equivalence.

PROF. DICKE: You are accusing me of not wanting α at all. . .

CHAIRMAN: Well sometimes I think α is slightly different than one.

PROF. DICKE: I wouldn't put it in these words at all. (laughter)

CHAIRMAN: That's what I think. It is fairly stated that we see no particular reason for putting up a big effort to make a test in a satellite of the atomic clock for the purpose of testing that α. Now, of course, if you do that in a satellite, the special relativistic effect will come in. I covered that in my initial remarks by saying that in my opinion at least, the special relativistic effects are best done on the surface. If this type of experiment were to be done in a satellite, I should think one would eliminate this term, or at least the effect of this term, and then throw it back so as to determine this one. I am now asking you if there is anyone who thinks that it would be worthwhile to make a rather considerable effort to put a clock into a satellite and get a further test of this effect.

PROF. DICKE: I am probably the one to make a case for doing something if anyone is. I have written some stuff. And my view is that it is rather an interesting question as to whether a clock there is the same as a clock here when compared with some metric measure determined in a particular way. But if you ask what kind of clocks you would like to compare, it seems to me the nuclear clock is ideal because it has electrostatic effects, ion interactions, and all kinds of complicated things going on, and if you don't see the effect on the Mössbauer experiment, I don't know where you would see it. It seems to me that the ideal way of checking for things related to Mach's principle as they exist along these lines is with the Mössbauer effect.

CHAIRMAN: I wanted you to talk next on the Mach effect.

PROF. BERGMANN: It seems to me that in conceiving the experiment you are not merely trying to determine the numerical values of these three constants, but the whole statement of general relativity, and if we question the theory we do not merely question the numerical values of the three constants of the formula, but, in fact, whether α is constant.

CHAIRMAN: You had better be careful. (laughter)

AUDIENCE: I don't want to see α at all.

PROF. BERGMANN: At any rate if you wish to check the numerical value of α otherwise believing in general relativity then you can't say that α is something that can be checked at all because in every experiment the product αm appears.

DR. THOMAS: Two things really come in here; whether you have a Riemannian metric and whether you have m in particular Einstein's law. You can have a Riemannian metric and still have something different from Einstein's law. Such would be the case if you had equations of this sort with a different value of α.

AUDIENCE: I think you have the problem of what you mean by the gravitational constant I think that's what Peter means, that eventually you have to define this. And if the αm comes in the ordinary weight of an object then one simply defines the gravitational constant to agree with it.

DR. DE WITT: α, I presume, would set the scale of the other constant.

PROF. BERGMANN: Perhaps we should question the theory on two different levels, first the principle of general covariance, and secondly that particular covariant theory we know as Einstein's theory. It depends on which level we consider it whether a certain type of experiment is worthwhile in terms of a major funding or not. I don't think we should, in principle, be willing to work exclusively with theory. We are hardly in a position to say whether we do or do not believe in such and such an experiment. If you would merely check the Mössbauer terrestrial experiment, at present it is such a good shape that merely to repeat the same thing as the satellite experiment is senseless. However, if you wish to check the theory at a different level, it is not inconceivable that the satellite experiment really checks something different from the terrestrial experiment.

CHAIRMAN: That's a fair statement. What I should state is this in line with Dr. Bergmann's remark. Is there an alternative theory for this frequency effect that should be taken seriously enough to make a considerable effort to put a clock up in a satellite and test it.

PROF. WEBER: I believe Wallensack's experiment gives the same results among all theories in which these calculations have been done in a consistent way, including theory of the Whitehead type.

MR. JONES: Isn't it true that in a case of a circular orbit at three halves the earth's radius the clock in the satellite remains synchronous with the clock on the earth? This seems to me to give the possibility of a null experiment.

CHAIRMAN: Yes, the special relativistic term cancels the other one.

PROF. POUND: There is no special virtue in running a null experiment. As far as that goes our experiment is a null experiment too. You can cancel it with a linear Doppler effect in this case.

CHAIRMAN: Bergmann, I had the impression that you were questioning this statement.

PROF. BERGMANN: Yes, I am. I would like to say first of all that as far as conceptual things are concerned, Dr. Schiff - I do not want to make this statement if he is not present. I want to say that in my opinion, the special theory of relativity and the principle of equivalence are not consistent with each other and therefore they lead to no self-consistent theory. If they did no one would have thought of constructing the general theory of relativity in the first place. Second, among the so-called flat space theories there are a number, among them that suggested by Hall back in 1957. This suffers from only one defect - it is about 150 pages long. At any rate he claims that one could construct a sort of Nordström isotropic theory that leads to a theory of gravitation. By appropriate selection of fudge factors you can get all the classical effects to come out any way you wish. This is an ideal theory, adjustable to fit any set of experiments.

CHAIRMAN: This is hardly enough argument to persuade Uncle Sam to spend yea many million dollars. Of course one can construct such theories, that has been proved by Birkhoff. Birkhoff had a theory that does all these things and in addition gives you the velocity of sound equal to half the velocity of light. (laughter)

AUDIENCE: The velocity of sound in a vacuum, I suppose? (more laughter)

CHAIRMAN: Prof. Schiff tells me that if we want to eat we should leave this room by 5 minutes 'til 12:00. The next item we had on the program was Dicke: "Experimental and Observational Tests of Mach's Principle." We would have at the moment only 20 minutes.

PROF. DICKE: Well, I can do it if you like, I can start and we can discuss this thing later. I think the one problem I'm going to have is that several of us just returned from Varenna where I gave a long series of lectures. If I am not careful I'm going to try to compress 6 lectures into 15 minutes.

CHAIRMAN: We'll let you know when you run out.

PROF. DICKE: Before starting to talk about Mach's principle I want to just take one minute to formalize certain things which I think we are probably all aware of. One is that there has been a serious lack of experiments in the origins of general relativity, and strong philosophical elements have entered in, and for this reason I think that one should perhaps be somewhat more suspicious than one is of such theories as quantum mechanics. It seems to me the only proper basis for a theory is a good set of experimental results. The next thing I would like to say is that if we think in terms of doing only experiments that lead to positive results such as the red shift or the perihelion rotation and so on, we leave out an important class which are the null experiments in which you really don't expect to see anything interesting and these often are the most important experiments. If you try to think back at the structure of general relativity, what is it really based on? I think the one thing it's based most firmly on is the Eotvös experiment on the constancy of gravitational acceleration, because it's the constancy of the gravitational acceleration, that leads directly to the idea that there are unique space-time paths, and this fact makes it reasonable to define these unique space time paths as geodesics in Riemannian geometry. So Riemannian geometry comes straight out of this experimental observation. It's a very important result I think. But I think there are a couple of other extremely precise experiments forming null results that have also played important roles. Some of these have come along only quite recently. I think the space isotropy experiment of Hughes is an extremely important thing on which we should base our consideration. This experiment and the class of experiments suggested by Coconi and Salpetre I think have been misunderstood in their implications. They have been thought to say something about Mach's principle. I don't believe they say anything about Mach's principle directly but, on the other hand, they do say something quite important and I'll perhaps get back to that a bit later to say what that is. The third thing that I think is an extremely important, extremely precise, experiment is the one that King has done on charge equivalence. This is not so important because of what it says about charge equivalence of positive and negative charges, it seems to me, but what it says about the velocity independence of charges. It means that you can't really tinker with Maxwell's equations very much and get away with it. When you can say that the charge - the source of the electromagnetic field - is independent of the velocity with an accuracy of the kind he has, this does not allow you to tamper very much with Maxwell's equations and try to get gravitational effects out as some lack of balance in the static interactions, or perhaps other things of this kind. These null experiments form an important foundation on which general relativity is constructed and I think there are other null experiments that can be done in the future with satellites that have not been done so far and which could contribute to our knowledge. If one can produce an orbit and calculate with great precision what you expect the orbit to be, the more accurately you can do this, and the more nearly the orbit agrees with what you expect, the better is the basis on which the foundation of general relativity rests. This can be done to ask certain specific questions, as you will see when I discuss this matter later. There is an important question of whether the active gravitational mass of the body, and by that I mean the source strength of the body, is a source of gravitational field. Does this depend on the mass distribution of the universe. The question is whether the universe of matter at a great distance plays a role in determining the active gravitational mass. If you start asking questions of this kind, then it's interesting to ask also whether, for example, the active gravitational mass of the earth as measured by a satellite going around it would depend on where the earth was in its orbit. We would expect from general relativity no effect. We would expect under certain other circumstances an effect. This is a null experiment; we expect no result but you would like to see whether you get no result. These are not the experiments which lead to front page news in the New York Times, and they get nobody any Nobel prizes but I think they represent the real hard core of the observational basis on which general relativity rests. Well these are just a few general remarks. I would like to say a little bit about the equivalence principle before talking about Mach's principle. When you read in the textbooks and you go back to Einstein's little book on the meaning of relativity you are somewhat frustrated to find the equivalence principle described in one particular way and used in another way in the theory. I think the best way to clarify the issue is really to find two equivalence principles which I would like to call the weak principle and the strong principle. Now the weak principle is the one you usually find defined in terms of the freely falling elevator. For experiments done in the elevator, the gravitational field disappears locally and the expected experimental results are equivalent to those out in free space. Or to say it another way, you expect the effect of the gravitational field acting on you to be the same as if the produced effect accelerated the laboratory upward. Now this is nothing but a restatement of the Eotvös experiment, that all bodies fall with the same acceleration, so if I drop the elevator and I drop everything in it, it all falls together. This is the way it's usually stated, but the way it goes into general relativity is somewhat different. This first statement I would like to call the weak equivalence principle. The statement is that bodies move on geodesics or goedesic paths if I leave out structure-dependent effects, such as interaction of the spin of the body or tight interactions with a large extended body. If I leave out such effects having to do with the second-order components of the metric tensor, then we would expect to have a path which is independent of the material of which the thing is made, or at least independent to a high accuracy. I remind you that experiments have only limited accuracy. A strong principle states somewhat more than this. It says if I go into a freely falling laboratory somewhere and I do some physical experiments, I arrive at some physical laws and both the qualitative and quantitative aspects of these physical laws will not depend upon where that laboratory is. If I do it here, if I take the laboratory out on Sirius, if I take it out in interstellar space, or if I had done it 5 billion years ago I would have always gotten the same results including all the quantitative aspects of the physical laws. And by the quantative aspects I mean such things as the dimensionless number like gm^2/hc representing the measure of the gravitational coupling. The assumption as it is used in general relativity is such that all physical numbers, go into a coordinate system which is locally Minkowskian, I describe the equations of motion that I see there in this locally Minkowskian coordinate system, and I then write down the unique laws which are the so-called laws of special relativity. It is always implicit in this that there are unique dimensionless constants that characterize these. This I would like to call the strong equivalence principle. Note that this statement says a good deal more than the fact that bodies fall with the same acceleration. Now there is an interesting question as to whether out of the observation that bodies fall with the same acceleration you can end up with a conclusion that the mass ratios of two particles to each other are independent of position or not. You see that a kind of argument can be made that if I have a particle with a mass m and another particle with a mass M, and supposing we define our unit of mass measure in such a way that the mass m is constant by definition. I want uncharged particles. I lift this uncharged particle of mass m in the gravitational field and I lift this one with M. If the mass ratio is different at the top, then you can see that some extra work will be required to change internal energy, and with that extra work you would expect an anomalous gravitational acceleration. One might infer because you don't see the anomalous gravitational acceleration that this mass ratio is the same everywhere. This is a strong argument and it sets very stringent limits on the kinds of theories that one can construct that will allow mass variations, mass ratios to vary. One should also note, however, in this connection, if you are going to use this kind of argument, that it says nothing about eliminating the contributions of gravitational self-energy. I don't mean the gravitational self-energy of particles, but I mean the contribution to the gravitational binding energy of a nucleus is completely negligible in relation to other binding energies; hence if I observe that a nucleus does not fall, that a nucleus falls with the proper acceleration, this does not allow me to infer that the gravitational contribution to the total energy of the nucleus, which comes from the interaction between nucleons gravitationally, is constant, and independent of position. In other words, I can't infer anything about the constancy of this particular number from the fact that accelerations are constant. This is something to bear in mind. Now I want to leave out of consideration the question of whether mass ratios of particles vary with position or not. I'm going to assume they don't. I'm going to assume that the fine structure constant is also a constant that doesn't wander around when you go from one place to another. But I'm going to leave open the question as to whether this particular number can be position dependent or not. As a result the considerations I'm going to make are going to be the ones which violate the strong principle of equivalence, and for that reason are not straight general relativity unmodified. It's my personal feeling that if you grant the strong principle of equivalence, then the general relativity comes out of the strong principle of equivalence like night follows day. But I would perhaps be beat down on that. I don't know, it's really quite difficult to see why not. May I withdraw this conjectural statement? In any case what I am going to consider later on is the situation tied to Mach's principle where this particular number will change, will be effected by mass distribution. Now in that connection I should point out that in 1937, I think it was, Dirac played a little number game, one of these numerology type of things which was rather interesting. Dirac noted that the dimensionless number (perhaps I will write it this way,) of gm^2/c is of the general order of 10^{-40}, that the age of the universe when it is expressed in atomic time units (and by the atomic time unit here, I mean the time it takes for an electron to go around once in the hydrogen atom), is an order of 10^{40}. And then if I go out to the visible limits of the universe, and take the mass of the universe which I see out to the visible limits of it and express this in say proton mass units, the mass of the universe divided by the mass of the proton, is a number of the order of 10^{80}. Then he said essentially the following: that a number of this kind we might hope to get out of a theory some day. People, when they see dimensionless numbers that come out of physics, I think divide into two camps. I'm not sure which camp has the biggest following. One thinks that dimensionless numbers like this fall like the gentle dew from heaven; you don't have to understand them - they have that value and that's it. There is another camp which feels that such a number should be understandable someday in terms of relation to other numbers, like $4\pi/3$ and if anyone has tried to construct a number of this kind out of 4π over three he gives up quickly. Dirac took the view that this number doesn't fall like the gentle dew from heaven but should be related to some other number and it seems to be related to these numbers. He took this quite seriously as meaning that if this number changes with time that this number should change inversely with time, that the gravitational constant should become weaker with time. I point out that this is a violation of the strong principle of equivalence and does not come out of general relativity, and that this number would change as the square of the time. I think there is implicit in this a statistical argument of a kind, that if you think that man could have lived almost any time from the word go until now and all times are equally probably in some sense then it's sort of strange that we would have lived at just that time where the age of the universe would give this number to agree with this so you might sort of argue that these should go together. I think the statistical argument, if it's a statistical argument at all, has a bad fallacy in it. It is that you don't have physicists around until you have enough heavy elements to make physicists (you can't make them out of hydrogen); and another thing is you shouldn't wait so long that all the stars have died of cold because physicists like to keep warm or else they can't compute anything. So if I were to draw a logarithmic time scale which extends from way over there, well this is infinity over this way on a logarithmic scale and I don't know how far this way, and 10^{40} say comes in here as $\hbar c/gm^2$ as a fixed number and then there is something which slides along here then I would say that physicists can only live from here to here and this is not such an enormous range of times. It is no factor of 10^{40} it's a much smaller range of times and for that reason one shouldn't be too surprised if physicists happen to live at the time when the age is about 10^{40}. My general conclusion from this would be that I don't think you can take Dirac's arguments too seriously. In other words, a very stringent cosmology which he cooked up in which this number changed with the square of the time and this linearly with the time, this inversely is the time, is I think not justified by the empirical evidence on which it is based. This would be my conclusion. Well on the other hand, I come right back to thinking that this is a number which could depend on the structure of the universe and might well change with time and I quit.

LUNCH: Thursday.

CHAIRMAN: Dr. Schiff has invited those who would be interested to tour the linear accelerator around 3:00 right after coffee break. The total break lasting perhaps a half hour. Bob I think we interrupted you.

PROF. DICKE: I think I am probably better able to carry on now after that chicken. Well, this afternoon I would like to talk about Mach's principle and gravitation. To say on the basis of what we presently know concerning the validity of these ideas and also to say what we may possibly find in the future from satellite experiments about Mach's principle. Now as a first step, I think I should define what we mean by Mach's principle. This must go back, I imagine to the ancient Greeks, but I don't know who to put the label on there. The problem is the picture which we tie onto physical space. We have to make a distinction between physical space and space in which mathematicians play with idealized points connected in certain ways. In the physical space we are dealing with a physical situation. And as far as I have been able to see, there have really been only two pictures of physical space that have come down to us. One is the notion of absolute space with an ether or some kind of material associated with it, a space having physical properties over and above and aside from those of the matter it contains. And the other is a physical picture in which you say that completely empty space is without physical properties and is without physical meaning and the only things physically meaningful are the relations of bits of matter to each other in space. We can think of this as a relativistic idea. The first picture appears very clearly, perhaps not for the first time but at least very clearly, in the writings of Descartes with his ideas about gravity as a vortex in a medium which was called the plenum and which carried the planets around the sun. It passes from him to Newton. Newton's physical ideas of space seemed to be connected also with some kind of medium but in the actual formalism he wrote down it appears as an action at a distance. This idea was quickly taken up by his contemporaries as meaning that gravitation was, for example, an action at a distance across space, which Newton always thought of as having absolute properties. So we think of Newton's space as an absolute space aside from the matter it contains. One is led then to the ether ideas of the propogation of light and the ether associated with Maxwell's ideas of electromagnetism.

QUESTION: Did Newton regard his absolute space as meaning anything?

PROF. DICKE: It is not completely clear from his writings whether he did or not, but there is one point in his famous statement that he doesn't make hypotheses, he merely makes mathematics, and in another place he has a statement that only an idiot would think that one body could act on another body across a void without any intervening matter playing a role. And it was very clear from this particular part of Principia that he had the same kind of matter-filled space that Descartes was describing. He certainly thought in terms of absolute rotations in the rotating water bucket experiment that there were absolute characteristic directions in space and that these were properties of space and were independent of the matter in it.

AUDIENCE: I think the whole point of that was that rotation appeared to be absolute but not position.

PROF. DICKE: I wouldn't argue with you on the question of position. I would just say this though, the physical picture he had, and some of his writings seemed to indicate this, was that matter filled with little balls of some kind. He would have taken the view I think that if you could really see these little balls you could tell where you were but it certainly never played a role in his mathematics. Mach's ideas first appear apparently in the writings of Berkeley who had some correspondence with Newton about Newton's absolute space ideas. And Berkeley had essentially the same ideas as Mach that in an accelerated system these inertial forces were from fields in the accelerated laboratory which one should ascribe somehow to the matter at a great distance because if the matter was not out there one would not be able to sense the acceleration of a curve. Even more than this, if you remove the matter bit by bit by bit until finally there was just one tiny little bright star with essentially zero mass you wouldn't expect these large inertial effects to be associated with that little bit of stuff out there. The actual mass distribution at great distance seemed to play a role in determining and producing the inertial effects. This concept that motion of matter relative to other matter is important and that the physical properties of space are derived from the matter contained in the space, the inertial forces observed in an accelerated system are to be thought of as having their origin in the rest of the matter in the universe, this we usually call Mach's principle. And it is rather interesting to read what Mach had to say about this. It's not a terribly clear statement of what we call Mach's principle. The characterization of inertial effects as gravitational effects associated with acceleration (there are all sorts of accelerations of matter in the universe) was stated rather clearly by Sciama in connection with a specific mathematical model, but I think the physical ideas are quite clear. Let us say, the sun is here and here is a small test body which is falling toward the sun in a universe which contains a great deal of matter at a large distance, let's say at a characteristic distance R space has only those properties which are determined by the distributions of matter in it and which we could describe in any set of coordinate frames we like: We can take a coordinate frame in which this particle is at rest and in which consequently the sun is accelerating this way and matter a great distance is also accelerating. If I do it at some earlier time if I'm going to think of the inertial effects as propagating gravitational waves, that in this particular coordinate system in which the sun is accelerated this way, and this test object experiences a gravitational force produced by the sun, there is also an inertial force we think of as generated by the rest of the matter in the universe. It is this particular way of describing the origin of the inertial reaction as a gravitational effect produced by the acceleration of matter at a great distance that we associate with Mach's name and Mach's principle. And an interesting thing, which I think Sciama pointed out for the first time is that the acceleration (relative to the sun) that you expect from the test object under these conditions is independent of what you presume the gravitational interaction to be. In other words, if I double all gravitational interactions I double most of these forces because they are both gravitational and there is still a balance in the force. In other words, that the acceleration is determined uniquely by a mass distribution and I do not have to put a gravitational constant in it to determine what the acceleration is. Another thing which Sciama points out is that the weak principle of equivalence comes out of this in the following sense. I put in some other kind of matter here, some other body; if these are both gravitational forces, they are again balanced. Thus I get a unique acceleration independent of the type of matter I put here.

PROF. SCHIFF: If you change the gravitational constant leaving the inertial mass the test object unchanged, then you do change the acceleration.

PROF. DICKE: There is no inertial mass in this way of formulating things. You see, all you have at this particular point is a body at rest, not accelerating in this coordinate frame in two forces. What we mean by inertial mass here is simply a measure of the inertial force which I have centered and this is simply a statement of this ratio of this active gravitational mass if you like is determined by this force and the inertial mass of this force is independent of what I put here. This is merely a way of describing it. Of course, it is a particular coordinate system; I don't have to use this coordinate system. I could use some other coordinate system. This is merely a convenient system in which to see this balance of an inertial reaction with the gravitational pull. Sciama's way of describing this was that it is merely a model of a theory, it's not a gravitational theory as such because it's based on a vector field which is not capable of describing the things you need. I noticed the other day that Weiskopt has made a discussion very similar to this using a tensor field though the arguments are essentially the same. If we choose a coordinate system in which the object is at rest we have the gravitational field produced by this object and we have also the gravitational wave radiated by matter accelerated earlier. You have to accelerate it earlier in order that you see now that it's being accelerated. It takes time for light to get here. Both the gravitational wave inflow and the light inflow is at the same time to arrive to produce the acceleration. This picture again is just a way of describing it because you need not use this coordinate system. Now of course everybody would say that this is merely coordinate transformation - not a real proper gravitational wave - it's merely a coordinate wave. But I think it's in the spirit of Mach's ideas, that he imagined that you set all matter moving in this particular way and the field that you would get is just the one described. In other words, it's a kind of active transformation that we normally think of as a passive transformation.

Now the interesting thing here is the notion that the acceleration should be determined by the mass distribution and one could use just simple dimensional arguments to get a value for this acceleration, a very rough value. If this is a mass m and this is a distance r then the acceleration would be (from things we have learned from Newton) proportional to mass divided by r^2. In order to have dimensions come out right the relation must depend only on the mass distribution and the velocity of propagation of the wave and must be independent of the gravitational constant. Then the only other expression which would look simple would be Rc^2 divided by the mass of the universe, and from this we get the usual expressions that everybody gets who worries about Mach's principle. The fact that this particular number (this is simply a proportionality, a rough value) gm/Rc^2 is a number of the order of unity expresses the fact, if you like, that this mass distribution leads to the acceleration needed to give you the right inertial reaction. One could imagine what would happen if you had some other mass distribution. If you took away all the matter in the universe or you reduced the amount of matter at great distance by a factor of 2, then this number would change, - it wouldn't stay constant. Everybody seems to agree up to this point; at this point there seems to be some disagreement as to what the solution is. We remember that Einstein was strongly influenced by Mach's principle when he developed general relativity. On the other hand, this is not an expression which comes simply out of field equations nor could we really hope to have it come out of field equations of general relativity alone. For one thing the field equations do not completely define a theory, one needs boundary conditions on the theory before you have a complete theory. And if the spatial geometry is to be determined by the mass distribution one would certainly have to introduce boundary conditions to do this. Some people think that the way to understand an expression of this kind is to say there are some presently unknown boundary conditions on Einstein's equations which permit only those mass distributions giving m over r the right value. I'm a little dubious about this solution to the difficulty of incorporating Mach's principle in general relativity because as an experimentalist I don't see what's to keep me, in my laboratory, from simply building a massive concrete shell about my laboratory and hence changing the mass distribution which I see in this laboratory and for that reason if I were to accelerate the laboratory relative to this mass distribution, I will find that this ratio has changed. In other words I don't see what the boundary conditions would be that would prohibit me from changing the mass distribution which I see locally. And it is characteristic of the equations of general relativity that the effect of such a mass distribution simply results in a flat space inside, and all flat spaces are equivalent in general relativity. The equations I see locally are characterized, and as soon as I say flat space they are unique equations. There is no observable effect I could get in the laboratory that would account for this massive shell out there. It is for this reason I'm a little dubious about getting Mach's principle into general relativity without any modification of the field equations. As I would interpret it, I put g on the other side or g to the minus 1, that if I manage to change the mass distribution of the universe, and change this (writes on blackboard) that the locally observed value of g would change in such a way as to give us a local acceleration which would mirror the change mass distribution. Now if this interpretation is correct it requires modification of general relativity, it requires modifying the theory in such a way that g is not a fixed constant but depends on a field of some kind. And I will just very briefly run through the kind of modification which we looked at and then discuss some of the observational questions associated with this. I think the first thing is worth remarking here that this kind of rough relation might well suggest to one that you have to do something like sum up the masses over the radius in order to get some measure of what the gravitational constant is. A linear theory would expect to lead to some such relation as that, but we don't have a linear theory here, so this is not mathematics but only say a semi-quantitative expression. However, it does suggest that the kind of modification we need is a theory in which there is some scalar which is determined to satisfy some kind of wave equation in which the measure of mass appears as a source, and this kind of wave equation is satisfied, because that would lead to the scalar from a fixed mass distribution being given by some such expression as this. Which would suggest that it is the inverse of the gravitational constant that must be related to some mass, to some scalar which is determined by a mass distribution. Now with these physical ideas, the next step is to see what kind of modification we can construct of general relativity. And you find first that you can't really get by with just the metric tensor alone, the reason being that there is no suitable scalar you can get out of the metric tensor that has these properties. The scalar curvature and all the other Riemannian invariants containing higher derivatives fall off from a stationary mass distribution with a power higher than r and, of course, contracting metric tensors give you 4 - not a very interesting number. For this reason it appeared that one has to introduce another scalar field. Now theories of this class were looked at quite awhile ago by Jordan, there is a whole class of Jordan theories in which in addition to having the metric tensor there is a scalar field. These theories were investigated because Jordan was very much interested in Dirac's hypothesis described this morning which concerns the coupling of the gravitational constants to the age of the universe. He tried to construct theories which in a proper mathematical way express this idea. Jordan's theories have been criticized by Fierz and by others because it was found that in order to get things that would give you a Dirac type cosmology, you were led to the lack of conservation of mass; you had matter created and an energy momentum tensor without a properly conserved quantity. The equations I'll write down do not have this property and they are very closely related to one particular form of Jordan theory. In fact, all one has to do is take the reciprocal of the scalar field that I will use and this appears as a scalar field in Jordan theory. This is the assumption that the equations of motion of matter are just the usual ones that one has in ordinary general relativity. Now let me just sketch this very briefly, then go on and discuss the results of this.

MR. JONES: Perhaps you can clarify one point for me. The action of the distant body in terms of the inertia of the single body seems to me to be an instantaneous action in the velocity of propagation.

PROF. DICKE: You see, if I suddenly accelerate myself now, I want to describe that in a coordinate system for which I am not accelerated but for which matter at great distance is accelerated. If I accelerate myself now, as for example, by rotating, I see the stars now swing across the sky. That means they must have swung across the sky much earlier because it took the light all this time to get to me and along with the light that arrived the gravity wave also arrived, the two arriving together. This is clearly a strange beast from a causal point of view. Because how did the matter out there know that it had to be accelerated at just the right time. One doesn't try to understand this causally.

MR. JONES: One more question, you do have the wave equation here and that does involve the velocity of propagation. Then this is not inconsistent is it?

PROF. DICKE: The velocity propagation of the gravity wave is equal to the velocity propagation of light, and the two arrive together.

QUESTION: This is not a gravity wave you are talking about?

PROF. DICKE: It's a kind of gravity wave. No, what I am going to describe here is now a new field which leads to the variation of the graviational constant. One thing I didn't say that I should have is that we are trying to understand from the standpoint of Mach's principle local gravitational masses and local inertial effects as being caused by the mass distribution. It is clear that the simplest way of thinking about this you would think, might be that the inertial masses of objects, of bodies, would depend on the mass distribution, that you wouldn't total the gravitational constant, that you would total the inertial masses. It is quite easy to convince yourself that there is considerable ambiguity and arbitrariness in the selection of things you want to remain constant and it is most convenient in terms of the kind of formalism you want to write down. I think you could define the inertial masses as constant and then you find that the gravitational constant has to change.

AUDIENCE: I think it's much better and more convincing if you have inertial masses. You just don't identify the inertial and gravitational masses.

PROF. DICKE: You can do it the other way, but if you do, you have nongeodesic equations of motion for matter. If the inertial masses were dispositioned then your equations of motion contain a term, or force, which comes from the inertial mass changing, and you can show that just a conformal transformation will take you from one case to the other. The metric tensor is different in the two cases. I think it's simpler to assume geodesic equations of motion. It requires a smaller modification of general relativity to do it that way, that is to do it in such a way that you have nongeodesic equations. But this is perhaps a debatable point.

PROF. BERGMANN: What you say is a matter of conceptual cleanliness. I would like to make a distinction in the discussion of Mach's principle and what you are about to say. What you are about to say is that the proposed theory is conceptually a complete construction. But when you talk of Mach's principle in an open and conceptual framework, one doesn't know what one is going to use as a basic concept.

PROF. DICKE: Yes, I think you are right, if what we mean as Mach's principle is the statement which Mach himself wrote in a little book, then we are in a great difficulty already, and I don't think we have a clear meaning of Mach's principle until you write some mathematics down. So Mach's principle probably means different things to different people. And for that reason I would like to state as clearly as I can what it means to me and get on with it.

PROF. BERGMANN: Is it not too late for this when you talk about those inertial effects of masses at great distances coming toward you? You have already assumed the existence of a metric which determines the speed at which these rays are coming toward you, which is obviously not what you want.

PROF. DICKE: Perhaps a better way of saying it is, one would think that if the inertial graviational effects did not depend on the mass distribution, then I could take the matter away at great distance bit by bit until I have nothing left but perhaps a few flashlights out there which are those fixed stars which Newton would have liked to use to tell him where his space was. Simply markers that tell you where your real physical space is, is not describing space in Machian language but describing absolute space. There is a little example that one can give that points up the real difficulty of incorporating Mach's principle of general relativity. I would like to just take a minute to do this. Imagine that I have this as a conceptual thing again, that I had swept my space free of matter except for a laboratory, all of my laboratories have to have smoke coming out of them, one physicist, some apparatus, and no matter outside of this laboratory, except for a bunch of ten-cent store flashlights put out here and are shining light beams essentially massless devices. Now we discover that this laboratory is fixed in an inertial coordinate frame. How it happened to get that way we don't know. But we have an ordinary type laboratory whose m divided by r is a small number which in other words doesn't influence the metric very much. r is any characteristic length defining the dimensions of the laboratory. We do experiments and we discover that all the laws of physics that are written in a set of books in the laboratory are satisfied, and the apparatus behaves in a quite normal way. The next experiment we do is we take a 22 rifle, we lean out the window, we fire the rifle tangentially so that a projectile travels off in this particular direction transferring some angular momentum to the laboratory and the laboratory rotates after this. Now we observe when we have done this that there is a gyroscope in the laboratory whose axis continues to point in a direction nearly fixed relative to the direction of the propagation of the bullet. It doesn't matter how far away this bullet gets, the gyroscope continues to point in that direction while the walls of the laboratory rotate around it. As seen in the laboratory this looks as if the gyroscope is rotating relative to the walls of the laboratory always pointing in the direction the bullet is going so clearly what we are describing here is a space, not in the Machian sense, because we would have to assume that this tiny little massless bullet was much more important in influencing the motion of the gyroscope than the walls of the laboratory were. This is what would come out of general relativity. This is clearly not a Machian situation. You have to either exclude it by boundary conditions of some kind and presently unknown, or else you would have to assume that the equations as we are writing them are wrong. We have to either exclude it or say the thing is not Machian. Maybe Mach's principle is not satisfied, this is a perfectly good solution too. Now let me get on with writing down in a very brief way the bare bones of the kind a formalism which does seem to be in accord with Mach's principle. These are the following: I hope that everyone will not go to sleep on this; I promise this will not take over 3 minutes. We get Einstein's equations out of a variational principle having the scalar curvature of space here, a Lagrangian density of matter here, and I am going to put in all those grimy constants because of the way we want to play with them. We usually write 16π times the gravitational constant here divided by c^4 times the Lagrangian density of matter times squared $\sqrt{-g}d^4x$. Out of this you get Einstein's field equations and you get the equations of motion of matter independently and in such a way that the energy conservation laws are satisfied. Varying with respect to the matter variables you get matter equations of motion; varying with respect to the g_{ij} you get Einstein's field equations. Now the modification I would like to make of this variational principle to get Mach's principle into it is to divide through by this g and turn it into a scalar field, call that φ, and then having introduced the new scalar field we had better introduce a Lagrangian density for it. ω is just a constant for this particular field. This is the usual thing we would write. Of course you can multiply by any function of the scalar. It's written in this form in order that this φ can have the dimensions of the gravitational constant. You just want this to play the role of the gravitational constant so that this can have the right dimensions and ω can be dimensionless. Now with ω dimensionless, we would expect that any reasonable theory would lead to ω being the order of magnitude of 1 if it's going to describe a Machian situation. If this is some odd number like 10^{-40} then I think we would have to assume that we haven't really solved our main problem. So the assumption is that ω should be the order of 1. Now, varying with respect to the metric tensor components, you get something like an Einstein equation; varying with respect to the φ, you get a wave equation for φ; and varying with respect to the matter variables, you get the usual matter geodesic equation of motion that you had before. So the matter equations, the matter variables, obey the same equations of motion that they do in general relativity. The only difference lies in the introduction of a new field and in the field equation satisfied by the metric tensor components. I'll write down these three types of equations: An energy conservation principle which incorporates the fact that the matter variables obey the same equations of motion that they do in general relativity and you have conservation laws of the same kind satisfied there; and secondly, the equation for φ, this is a contracted energy momentum tensor of matter as a source term; and three, the Einstein field equations (writes equation). Now I'll discuss this last equation very briefly, that side is perfectly normal, a regular Einstein left side. This term appears perfectly normal except for this variable gravitational coupling that appears in here, this constant φ^{-1}, plays the role of G. This term is nothing but the energy momentum tensor of that scalar field, which we should expect to come in, also with the variable coupling, there is also an extra φ^{-1} in front of this. These last two terms are rather odd terms. They come from the fact that you have to integrate the second derivatives by parts in this and variation with respect to the g_{ij}'s, and leads to these two terms. The two terms play an important role because if you take the divergence of the left-hand side you get zero as an identity and it turns out then that the divergence of this term cancels the divergence of this in such a way as to lead to the divergence of the energy momentum tensor, the latter alone being zero. So that you get energy, you get conservation of matter locally in this theory as you do in general relativity.

QUESTION: I see the divergence of the last two terms depend on field equation number 2.

PROF. DICKE: Yes, it depends on this. Just as in general relativity you don't have an energy momentum pseudo tensor appearing in this tensor equation. In other words gravitational energy doesn't appear as a proper tensor expression in this any more than in general relativity.

Now I think the next thing I should do is to show you how this is capable of doing some of the things we might have hoped that the Mach principle theory would do. If I take a point in a space filled with matter at a great distance, I bring in some of that matter and produce a concrete shell which is closer, having a mass m and a radius r. We can ask for the effect of this in changing the gravitational constant seen in the interior. The effect on the gravitational constant is this.

PROF. TAUB: How do you supplement this with boundary conditions - solve the boundary conditions in general relativity?

PROF. DICKE: First of all I should say this about boundary conditions, that the boundary condition problem hasn't been solved generally here any more than in general relativity. One has to make some definitions about boundaries and boundary conditions. The boundary condition problems are discussed for this theory in a proper way for only one case and that's a cosmological solution - only for that one case. There is another case of a static mass shell (Interruption, PROF. TAUB: You will have to know something about boundary conditions before you can bring the matter in.) I will say what they are: In assuming matter at great distance in such a way that the scalar goes over to φ_0 asymptotically because of matter of great distance without trying at this point to say what the matter is. I would have to go into the cosmological solutions to see how this φ_0 is related to the mass distribution.

This has been done. The cosmological problem has been solved in such a way as to incorporate Mach's principle into it. The φ you see in the interior then for this is equal to $\varphi = \varphi_0 + 2M/(3+2\omega)c^2R$, and you will note that this is the inverse of the gravitational constant. Well, it isn't quite that as a matter of fact because if you look at the weak field solution, you find that the true gravitational constant is not the inverse of this but just multiplied by some simple function of this with the ω in it. So that what you actually measure is not φ^{-1} but it is some number times φ^{-1}. (Writes equation on board) So we see that the effect of this matter that we brought in has been one of reducing the gravitational constant, making it slightly smaller. You see then that it's possible with this theory to have some understanding of why the gravitational interaction is so weak, it is so weak because there is so much matter at great distance that is contributing a large term of this type which has been cranking down on the gravitational constant making it smaller and smaller. In other words the reason gravity is weak from this point of view is because of so much matter in the universe. Now the next thing, let me go back to the case of the rotating laboratory, where I fired the rifle and set the laboratory rotating. Let me idealize that by assuming the laboratory is in the form of a spherical shell, again with mass m and radius r, and this is set rotating with a certain angular velocity. And then lets ask what the precession of the gyroscope is inside this. I have matter at great distance of certain mass density with a real cosmological solution. This is the Einstein-deSitter flat-space solution. Now I will write down the explicit form for the weak field approximation. This is only good in the weak field case for the precession of the gyroscope inside. Now this is nothing but the analog of this 5 here, the Lenze-Thirring precession of a gyroscope due to the rotating of a mass. It's just these same equations written down for these field equations and it leads to this, that the rate at which a gyroscope inside precesses is equal to $2(3+2\omega)/3\pi(4+3\omega)$ times the mass of the shell over the radius, times the density of matter in space now, times the age of the universe now, times c^2, times α_0. Now let me see again what this is. I'm assuming a uniform universe, isotropic, of the Einstein-deSitter type, with the cosmological equations written in such a way as to include these field equations, and the connection between the rate at which the gyroscope precesses inside relative to the rotation rate of the mass around it is this angular velocity relative to that. You see that it's characteristic of this. Now, if I imagine conceptually that I took another universe with less matter in it, i.e. I reduce ρ_0, α would increase, and you can imagine if the equations were valid in the limit as this gets smaller and smaller, this gets larger relative to this, that the limiting case might well have this rotating at the same rate as that. However, that limiting case has never been investigated because this problem has never been investigated under the conditions beyond the weak field approximation in this mass to radius ratio. It's only valid in that weak field case so you can't really look at the limiting case so it does seem that these field equations are better able to incorporate Mach's principle in the sense that I had defined it at least than Einstein's equations are. Now what are the general expectations that are associated with the law of physics if these equations are satisfied. First of all, as the universe expands we expect the gravitational constant to get weaker with time. The gravity gets weaker as the universe gets older. The next thing is that if I approach the sun and measure the gravitational constant, it ought to be smaller than it is out here. This is because of the influence of the sun in reducing the gravitational constant and in the same way that the spherical mass shell has reduced the gravitational constant inside. These are the two main expectations, the gravitational constant would be affected by the mass distribution in this way. Now its interesting that there are a number of satellite experiments, a number of ways of getting at this kind of hypothesis to see whether it's valid. One thing I forgot to mention is that these equations satisfy the top condition exactly, the deflection of light is a slightly different value depending upon what ω is and if we had any accuracy on that there would be a way of finding limits on ω. There is also on a perihelion rotation a slightly different value depending on ω, and I have included on the basis of looking at the data, and so on, that ω should be greater than or equal to 6 if we are not to get into troubles with the perihelion rotation. I'll bet that other people would differ with me and would say that it would have to be greater than 6 but that's my own personal conclusion. So this does not give the three standard tests except in the limit of ω going to infinity. If ω goes to infinity with this theory it reduces to Einstein relativity; hence, it differs only for finite ω. One obvious test then is to set better limits on ω by doing a perihelion rotation with a satellite experiment somehow. Without trying to say how this is done, I think there are very serious troubles with trying to do a perihelion observation on a satellite; but let's just say this is the area where something could be learned. Another thing would be if we had a gravitational clock that we could compare with an atomic clock as time goes on. The gravitational clock should run slower and slower so one test would be to put up a time keeping satellite. A satellite at high altitude where the gas damping is rather small and to see whether a satellite takes longer and longer to go around as gravity gets weaker. Let me write down what I think the order of magnitude variation to be considered is. It's about 2 parts in 10^{11} per year a secular variation, gravity getting weaker by about 2 parts in 10^{11} per year - a very severe requirement to be met in terms of observation if one is to see this. Another effect is if you have the earth going around the sun on elliptical orbit and you have a satellite going around the earth, then the earth gets closer to the sun in December than in July. For that reason there should appear in this a periodic effect with the period changing with the annual period, the period of satellite increasing and then decreasing, as the earth goes around in this elliptical orbit so that this effect should show up. This is probably a very nasty one to try to find because of the effect of all the other annual period perturbations that come into the motion of the satellite. I would guess that this would be extremely difficult to do anything about. Well these are two experiments that one could try.

PROF. TAUB: Will you indicate on what basis you make these predictions? Do you take $\varphi = 0$ and solve the field equations?

PROF. DICKE: Well I haven't discussed the cosmological solution of these equations with the boundary conditions that mirror Mach's principle in a proper way. Let me say what I have done about that. It is to assume that if you had an expanding universe, expanded from a certain time on, that the value of φ that you get depends on an integration of the light cone in the past to the matter distribution that you see, that you don't have a surface integral that comes in at the start. There is no contribution for surface integral. All you see is the contribution from the mass in the past. That's the unique boundary condition, an outgoing wave boundary condition that determines what φ is from its wave equation.

AUDIENCE: Is there a T_{ij} in these equations?

PROF. DICKE: The T_{ij} is in the cosmological conditions, I assumed a dusty universe. Nothing but mass particles, no pressure.

AUDIENCE: And you apply that to the sun by doing what to the T_{ij}?

PROF. DICKE: Oh, you mean to discuss the Schwarzschild case? What is done for the Schwarzschild case is this, you write down the analog of the Schwarzschild solution for these equations exactly.

QUESTION: Is T_{ij} equal to zero then in equations (1), (2), and (3) that you have written?

ANSWER: In the region where you are considering the metric tensor, you mean? Outside the sun? Yes. It is zero.

QUESTION: But φ is not zero?

ANSWER: φ is not zero.

QUESTION: Is it a static φ or is it a time-dependent φ which is a solution of 2?

ANSWER: It is a static φ used for discussing the perihelion rotation. It is a mass source, a matter at great distance, and the whole thing assumed to be static. You are only interested in the region close to the sun and you are interested in a time sufficiently short that you don't need to worry about secular changes in these short times. Anything else? I can write down the Schwarzschild solution exactly if you like. Let's put it this way I think these equations are slightly more complicated that Einstein's equations and whether you can do that in a simple way, well, it isn't that easy. One thing I should have said is that what I am describing here is primarily the work of Bruns, one of my students, and that this theory, as I said before, is very closely related to one of the particular cases of Jordan theory. The Jordan theory has a particular value for the parameter of this theory and with the replacement of his scalar field by the reciprocal and with explicit statement about what you mean by the metric tensor by the matter equations.

DR. ROMAN: I want to ask about the second of the experiments that you describe. Is what you want to compare, winter and summer, the period of the orbital rotation of a satellite? Should this check independently of the semimajor axis of the satellite?

PROF. DICKE: They are of course coupled together.

DR. ROMAN: That coupling is still O.K.?

PROF. DICKE: Angular momentum is conserved in this so that the satellite keeps the same angular momentum, but if you make gravity stronger it gets into a smaller orbit and goes around more rapidly. The usual assumption one makes in general relativity is a uniform isotropic universe with matter in it and that pressure is small. You can discuss all three cases, open, closed, flat. It's the usual thing you see, just the field equations are a little different. You have in addition to this, a new field variable which changes with time.

QUESTION: Then the T_{ij} you do not see?

PROF. DICKE: For the universe, no. It's uniform and has a value characterized by the mass density. Any other questions at this point?

QUESTION: You haven't really made an example of experiments that might actually be performed.

PROF. DICKE: I'm not sure that these can't be performed. I don't know that they have been looked at closely enough to know whether these are feasible or not.

PROF. POUND: How about the earth period about the sun, or the moon period?

PROF. DICKE: The moon period is perhaps better because it goes around more rapidly. It has a bigger angular velocity. And the problem there is, that I don't think we can do better than one part in 10^8 in a year, which is perhaps if you wait 10 years getting in the right ball park.

DR. ROMAN: What sort of effect do you expect on the period of the satellite?

PROF. DICKE: Well the order of 2 parts in 10^{11} change.

DR. ROMAN: Essentially the same number.

PROF. DICKE: Yes, I think it turns out to be essentially the same number for some odd reason.

QUESTION: Does this number actually enter a couple of times?

PROF. DICKE: A factor of 2 either way maybe, but the reason I write that down is if the number were far larger than this, you run into violations of certain observational situations, namely, in particular the perihelion rotation gets you into trouble; the second thing is that whatever evidence there is on this leads to this kind of a number which is in connection with evolutionary rates of stars. There is some indication there that you can explain the problems there are in this particular way and this leads to this particular value. So I don't think that this is in any way a firm prediction of what one should look for but I think it's in the right ball park. I know it can't be far larger than this or there are real troubles with the stellar evolutionary rates.

QUESTION: The figure you quoted on the moon time is that what you think can be done or is being done?

PROF. DICKE: About a part in 10^9 is what they are doing now on moon time; perhaps someone knows better that I do what this is. But I think the moon camera and the analysis of this gives you time to about 1 part in 10^9 for the year.

STATEMENT: It seems as if one could advance a lot from that.

PROF. DICKE: One would hope so. There are some peculiarities about understanding moon's motion as I understand it.

QUESTION: What would be the effect of putting a satellite around Mercury?

PROF. DICKE: It sounds to me that this might be orders of magnitude more difficult than to put one around the earth.

STATEMENT: It would be hard to see it very well.

PROF. DICKE: Yes, there is an observational problem.

QUESTION: How does this depend on ω?

PROF. DICKE: It depends on whether I'm talking about a closed universe or an open universe, but if I take ω to be 6, and take the Hubble age that we see, then it turns out to be about 1 in 10^{11}. For the closed universe, the kind that looks like things fit well, this is about 2 in 10^{11}. This is the secular change in the gravitational constant parts per year.

QUESTION: I thought for these experiments the T_{ij} was zero; with the Schwarzschild type solution it went to zero.

PROF. DICKE: We are discussing completely different sets of problems here. We were discussing the problem of the perihelion rotation, and there I was using the Schwarzschild solution. This number comes out of a cosmological solution with the observed Hubble age of say 13 billion years, observed last year. I'd hate to tell you what it was observed 10 years ago. But last year's value of 13 billion years is more like 10 by now.

QUESTION: Presumably if it has a bearing on satellite experiment, it has to be fitted to the Schwarzschild solution.

PROF. DICKE: Yes, what you would do there, I think is quite clear, is to fit the Schwarzschild solution on the cosmological solution at great distance. The Schwarzschild only has to be carried to the second order in the g_{44} term and only the first order in the other terms, for purposes of discussing these motions.

QUESTION: I take it this thing is inverse to ω?

PROF. DICKE: Let me write it down for the flat space case. I have an explicit value for it. Now this is the Einstein-deSitter universe, R is just a parameter and φ varies with time in this particular way, where t_0 is any particular time, like time now, and this is the value of φ now. I have some graphs of the way this parameter goes with time. I don't know if you can all see them. This graph, the middle one is a flat space and the one up here is a closed space, and the one down here an open hyperbolic space where t goes with time. These are obtained by numerical integration. Now let me continue a bit with the observational situation. The question whether gravity has been changing with time or not. This is a matter which was first discussed in a paper by Teller, I think shortly after the war, I don't remember exactly, in which Teller criticized Dirac's cosmology on the grounds that the dinosaurs would have been broiled to a crisp. A very interesting suggestion if Dirac's cosmology were satisfied. At that time the Hubble age was so short, and with gravity varying inversely with the time one knows from stellar dynamics that the sun would have been so hot at the time the dinosaurs were living that they would have hardly found it a very comfortable earth to live on. Since that time, the Hubble age has changed somewhat. In other words these changes have gotten so slow with time that this is no longer valid. Objection?

QUESTION: Did Teller take the constantly changing radius?

PROF. DICKE: Yes. There are a number of geophysical and astrophysical effects associated with changing g. If it really occurred, it should have rather important influences on the history of the galaxy and the solar system and the earth and one might have thought as a result that you could simply look to see what the situation is, and hence decide whether the gravity has been changing or not. I've tried hard to do this. I found it extremely difficult to really make any firm conclusions of any kind. The earth is such a complicated thing that as soon as you decide that the gravity changing in such a way would have such and such an effect, you will discover that there are about three other ways of explaining the same effect. The result is that it's quite difficult to draw a firm conclusion from these things. At the same time one realizes that if it were happening it would be quite important. Now we mentioned the problem of a time-keeping satellite in comparing satellite time with an atomic clock. Well there is another way of getting at this which is to make use of the fact that the earth rotating on its axis has been an atomic clock in the past, because dimensions are determined primarily by strong interaction rather than by the gravitational interaction and if you compare the moon time, obtained by the moon going around with the rotation of the earth on its axis you could get some idea whether this has happened or not. Well, there is a very nice book which Munk and McDonald have brought out on the earth's rotation and if one looks in this he discovers that you can do a reasonably good job of accounting for all the rotational effects that you observe. The earth is rotating on its axis but you find when you are all done, the earth apparently has been speeding up with time in an unknown and unexplained way, and the speed-up rate, the earth apparently going faster with respect to the moon time, is just what you would get from this kind of a number. So that agrees beautifully. Well one might say, "Well, this is wonderful but life isn't that way." The earth is a complex thing and it turns out that if the sea level had been falling with ice piling up in the arctic region at the rate of about a meter per thousand years, it would mean a 2-meter drop since the time of the ancient eclipse observations were made. On this analysis you would get the same effect. Well you would think you could simply look at coast lines and find out whether the sea level has been going down or not but this apparently gives ambiguous results. Some places it looks like it has been going up and some places down. So there is nothing very much we can get out of this. Other effects are also equally difficult to pin down. The general expansion of the earth that you would expect with gravity getting weaker seems to be completely lost in continental drift and mantle circulation effects if they exist. One thing which is quite predictable which does look interesting is the effect of this on stellar evolutionary rates. If gravity was stronger in the past, the stars evolve more rapidly and this leads to the apparent ages of stars, the old stars being much older than they should be and I'll write down a table of numbers of the various ways of dating the galaxy and objects in the galaxy and see what the effect on these numbers of putting in this hypothesis is. For globular clusters, this is rather a poor number which has been handed down by word of mouth. I don't know whether this has changed by now or not (they change rather fast), but the age of globular clusters is about 25 billion years and perhaps there are experts here that know of a more recent number than this. There is a number due to appear in a paper which Sandage has written and is being published.

PROF. FOWLER: Arp is writing a paper which is 20 plus or minus 4. It's on M5 but he says in the paper that M3 and M2 ought to be very much the same.

PROF. DICKE: I also heard a rumor of 30 for one. Do you know anything about that?

AUDIENCE: There have been lots of rumors. What you have to read is what they submit for publication.

PROF. DICKE: You don't know whether publication in the New York Times constitutes a proper publication?

PROF. FOWLER: Robertson is right; that's the age of M5.

PROF. DICKE: My computations are based on M5. Let's remember this 20 plus or minus 4 here. I'm surprised at a plus or minus 4 because I would have thought it would be a much larger error than that. Then there is an old galactic cluster NGC 188, with an age of 16 billion years, and the sun. Now in the sun we have to distinguish between various kinds of ages. There is the age you get by dating meteorites; there is the age you get from stellar evolution, simply looking at what you know about the sun. Schwarzschild would put this somewhere between 4 and 15 for the stellar evolutionary ages, and for the meteorite age he would say 4.5.

PROF. FOWLER: There is a paper by Lindblad which gives 12. That would be the age when the sun becomes a giant but it's based on the assumption that the sun at present is 4-1/2 billion years old, so it's related.

PROF. DICKE: Apparently the sun is tremendously uncertain because of the fact that it is still on the main sequence and we don't know the helium abundance in it. There are no really good measurements of helium abundance. Now in the case of elliptical galaxies, for the evolutionary ages there is a recent paper by Hoyle and Crampin which places these between 10 and 16. This is kind of rough because this is just over-all color measurements, trying to match the colors of these things with the color of one of the galactic clusters. This is for ellipticals and then there are the so-called Wilson-Oke stars, I don't seem to have those on there, but they were about 15 too. This is a very neat and clever way of dating individual stars with just normal field stars, you get all kinds of ages but they run up to about 15 billion years if not greater. Another way of dating the universe, dating the solar system is in terms of the uranium that it contains and this has been analyzed by Hoyle and Fowler. And Fowler will object to what I am going to write down but nonetheless I am going to write it down. What I am going to write down is the age of uranium based on the following assumption that when the galaxy is first formed you found a lot of stars of halo population. This population generates quite a bit of heavy element in a very short time and after that the relative abundance on a fractional basis of heavy elements increases linearly with time. I'm going to assume two cases, a 25-percent prompt production and a 50-percent prompt production. And these get for the age based on uranium alone something between 7.5 to 11 billion years. Then there is the age one gets for an evolutionary universe based on the assumption of a Hubble age of 13 billion years and you get 8.6 billion years for the universe, for a flat space and something less than this for closed space, and up to 13 for a hyperbolic universe. I think these are the principle kinds of ages that one has to discuss. And you notice rather bad discrepancies here, with stars older than the universe by quite a bit, by a factor so large that it's a source of worry. Now I'll show you the effect of putting in a variable gravitational constant - this is for an assumption that ω is equal to 6 - down here with a closed space with a present radius. I can do it for a flat space just as well. Let me write it down. ω 6, flat, and this is 8.4, 7.6, 3.1, to 7.3, meteorite age is unchanged, elliptical galaxies change down to 5.9 to 7.6, Wilson stars are about the same thing, 7.5, and then this stays the same and this. Now you note that those numbers are in agreement with each other. Because this is the age of the universe which is 2/3 of the Hubble expansion age, they are assumed to be the same, I think it is slightly different, but it's very nearly the same. Let me just see what the number is. Yes, it is actually a little different; it's 8.3. Now I don't know what you will conclude from this except to say that if it should turn out that these numbers are not terribly bad, if the Hubble age doesn't keep changing from year to year in a nasty way, and ages of globular clusters should settle down and stay this way, this could be quite significant someday. It is not terribly significant now I think because of the past history of these numbers. The difficulty of inferring ages of globular clusters is quite severe because of the way they brighten up the main sequence and the way they turn off. This probably has less uncertainty than this one.

CHAIRMAN: What was that based on?

PROF. DICKE: I don't know about the Hubble age; the Hubble age has gone up and now has started coming down again. It looks like a turning point because it's been monotonically increasing for the last 15 years.

PROF. HECKMAN: It's very complicated but I just read a paper by Hornbeck at Upssala who discussed systematic errors in galactic velocities and this point up to now has not been discussed by Sandage and his colleagues, who see very small systematic errors which depend on the brightness. If you try to correct these you will come down with the present expansion, once more going up by an amount of about 30 percent. If they add it might be 12 or so. But I don't know whether other astronomers would agree about the points they discussed. I only know it's a personal opinion. But would you allow me to make a remark in this context? (Yes) I feel always that if one contrasts these numbers in your left column with the age of the universe that in this case one takes very seriously the isotropy and the homogeneity of the old models which are being considered; and in all these the Einstein-deSitter models are of highest simplicity, even primitive let's say, from the standpoint of cosmical hydrodynamics. We know of these inhomogeneities in the universe, and until now nobody knows what will happen if we extrapolate to the present distribution of

Mach system in space backward in time; it is by no means sure that the focus is to one big bang. I can say with certainty, that the work of Lipschitz and Landan, I forget at the moment some names, but I can certainly say that all these investigations show that the inhomogeneities are growing if you go into the past. Nothing is smoothing out when you go into the past - everything is exaggerated. The difficulties are increased if you simply extrapolate this simple solution. And in the very special case of Newtonian Cosmology, which is only a rudimentary substitute for relativistic cosmology I confess, you can show that you can easily build models which have a bottleneck through which clusters can go without being disturbed; so the similarity of the big bang can be made to disappear completely. Nobody knows that such solutions exist also in relativity. So I think we should not exaggerate the contradiction. It's nice to have the possibility to come down with these numbers, but the contradiction need not be so serious as it is thought very often because these models which must be considered in their extreme idealized homogeneity certainly do not correspond to the present situation, and if you extrapolate them to the past, the big bang might assume such a complicated character that there is not one singularity. There may be isolated singularities. I don't know what the factors are in that case; nobody has studied this as far as I know. I want to dilute the seriousness of the arguments.

PROF. DICKE: Well I don't know that the arguments are very serious in any case, because I think the history of these numbers is such that one should not take these things very seriously. But I think it is interesting that there is no contradiction at least, that the numbers are made to agree with each other more satisfactorily if you take this theory with a Machian approach, just as in standard general relativity. I think our time is running on to the point where I had better sit down.

PROF. FOWLER: I would like to just say that you must emphasize from the uranium radioactivity age, you cannot change that by changing the gravitational constant. As you know when I first calculated that I got 15, and at the present time my calculations give a value more like 20. I grant you that this depends on a great deal of argument about how radioactive elements were made, but when you have changed that number in a different way, you have changed the others.

PROF. DICKE: There is quite a little argument on this, but there is no effect of changing gravitational constants by changing the radioactive decay rate, that is, a fixed radioactive decay rate. We differ from each other in primarily two things, I think. One is whether you make a sizable piece of the heavy element content in a very short time in the halo population and the other thing is whether you want to wait a long time before you start making uranium because that is made in stars with a rather long life.

PROF. FOWLER: The other point is, I think it's only fair to say about the red shift measurements that the reciprocal of the Hubble constant, so far as the group at Mt. Wilson and Mt. Palomar are concerned, has not changed very much in recent years - that's 13 billion years and Sandage has been sticking to that ever since the original expansion by Baade. One must not say that this has been the capricious desire of a group of people to just change the number. There are good solid observational reasons on which this is based. So the 13 is a good number. Then to get 8.6, you have to look way out at the end of the line at the very most distant clusters and ask "What is the universe really like? How do you draw all the dr/dt's back to the time of the big bang?" Assume 2/3 or $4/\pi$, or whatever model you use, and the critical galaxy. The observations on Minkowski's critical galaxy can give some hope of distinguishing between the models. It can be anywhere from practically 4 to infinity. You cannot assume the q_0 less than zero, so you might want to use models with a cosmological constant.

PROF. DICKE: The observations favor a closed universe now but they were so poor . . .

PROF. FOWLER: They favor plus 1 for the acceleration parameter, but the spread in the magnitude of that critical cluster was such that you can have $q = 0$. You can compute for $q = -1$ and you can compute for $q = +3$ with one magnitude variation in the luminosity of this very very distant galaxy. It seems to me the best number to write is 13.

PROF. DICKE: That number was written down as an explicit calculation for a flat space without any assumption of any observational justification.

QUESTION: If the ω is something smaller, would that have a profound effect?

PROF. DICKE: It makes the effect bigger. You get a bigger variation of perihelion rotation.

DR. HECKMAN: In your formula concerning this quantity α - I consider this second bracket for the moment - am I right in saying that r is a function of the age of the universe or not?

PROF. DICKE: The r that's in there is the nucleus of the mass shell that you are rotating. The universe parameters are the age of the universe here to the density of matter.

DR. HECKMAN: Yes, but if you have not extrapolated it for a cosmological model, then m is increasing and r is increasing as you are approaching.

PROF. DICKE: This m is not the mass of the universe it's the mass of this shell, this local mass shell that you have built.

DR. HECKMAN: You have never tried to apply this formula to the universe as a whole?

PROF. DICKE: This formula is meant to hold the following situation of a uniform universe, with a mass density ρ and age t, and Einstein-deSitter model. And in that universe we build a spherical mass shell laboratory with a mass m and a radius r, and we set the thing rotating. Then we asked what the Lenze-Thirring precession is of the gyroscope inside, the ratio of precession to this.

DR. HECKMAN: You never mean to apply this formula to universe without this?

PROF. DICKE: No. It's only meant to hold for that.

DR. HECKMAN: What about the Salpeter-Cocconi effects?

PROF. DICKE: I don't think we have time now to discuss the Salpeter-Cocconi things.

DR. DE WITT: I think it might be worth pointing out in connection with the theoretical aspects that this may not be such a completely ad hoc theory as it seems, I get the impression that very similar equations come out of the generalizations of some of these old untried theories where you take the fifth dimension seriously.

PROF. DICKE: Let me say that the generalization of the five dimensional theory gave you electromagnetism as part of the formula. You just lose that (that's exactly what Jordan said).

AUDIENCE: Is that all he said?

PROF. DICKE: Yes and for that reason electromagnetism is brought out as a special field you see; he never talked about the other matter variables.

DR. DE WITT: With the extra scalar here, that is just about what happens here.

CHAIRMAN: Might I remark as many people have done, this Lenze-Thirring thing if you apply that in a most naive way to not a shell as here, but to a solid sphere, it can have a hole, this number here turns out to be very much like that sort of thing over there, when we replace this m by $4\pi d^2\rho dA$. Then you get the formula that gives you here essentially gm/rc^2. That is the same kind of numerology that one can arrive at with the Dirac theory or is implied by this theory too.

PROF. DICKE: Does that assume a particular mass density in space?

CHAIRMAN: Yes. I'm not defending it as a serious matter but just by putting that little hole there and applying Lenze-Thirring you can get it. Of course, I know Taub would be distressed about the boundary conditions which I am too.

COFFEE BREAK: Afternoon on Thursday.

TRIP THROUGH LINEAR ACCELERATOR.

DR. SIRY: Paper entitled "Determination of Position and Velocity of Artificial Astronomical Bodies." (Thurs. afternoon)

Well, sir, I think I might just review briefly the methods that are now used to determine the orbits of close earth satellites since these are the most highly developed and I'll also say a few words about the methods that have been used to track some of the things that went out toward Venus.

There are really just two types of observations that are now available for precision work and in fact we might even limit that to one. Those used most frequently initially are the radio observations that come from the system known as the Minitrack system with which you are probably familiar. The technique used is that of the radio interferometer. One has the usual sets of antennas to get the one component of the direction of the satellite. Then there is a second set to get the other component and then there are numerous ambiguities in resolving the antennas to take care of that problem. This system of course works in conjunction with the transmitter of the satellite. In other words, we have to start with the radio observations and in fact we consider this as the first building block. Now the other type of observation that really has the most promise is optical observation, and the network that's now used to get these is the network established by the Smithsonian Astronomical Observatory. This is the network of the ten Baker-Nunn cameras established in a roughly equatorial belt around the earth between the latitudes of 30° North and 30° South. Of course, the Baker-Nunn system utilizes the standard astronomical techniques. One obtains photographic plates and measures them with measuring engines. There are extra difficulties, in this case, associated largely with the timing. The satellite motion is an order of magnitude greater on the angular rate than are the motions of the stars and the timing problem is a more severe one, and it has led to the result that position along the arc could be determined with the uncertainty of the order of seven seconds of arc, while position normal to the arc could be determined with an uncertainty of the order of only a couple of seconds of arc. The Minitrack system, the radio system, is calibrated by practically the same techniques. In other words, there is a camera at the electrical center of the system which is used to photograph flashing light in an airplane against the star background and, in effect, coincident with this flashing light is the radio source. So that at the instant of the calibration the Minitrack system is potentially as accurate as the Baker-Nunn system. The difference is, of course, that the calibration is only performed every few months and that, of course, there are electronic drifts and things like that that tend to decrease the precision. One of the important things to keep in mind in any discussion of the accuracy of observations is the following: I referred to this figure for the uncertainty in a Baker-Nunn observation which is, of course, related to the uncertainties involved in measuring the plates, and the uncertainties involved in the timing. Now the actual process of determining an orbit again follows classical lines; different corrections are performed with respect to all the observations that are made during a certain time interval. It is customary now to take an interval of the order of a 100 revolutions or perhaps 200 or 300, so that in other words, the arc is a week long, or several weeks long. Instead of being faced with the problem of the residuals with respect to an initial track along a plate for an individual pass by a single radio Minitrack station, what we are, in fact, faced with is the problem of evaluating the residuals of the observations with respect to the whole orbit. And the number that comes out there is considerably larger than the numbers you associate with the individual instruments. As a matter of fact, for the immediate post-flight work it's the order of a hundred seconds of arc for both types.

DR. ROMAN: Are these closed orbits to the accuracy with which we can work with them?

DR. SIRY: Well, it depends on what you mean by a closed orbit. It's not closed in the mathematical sense.

DR. ROMAN: I was thinking in a physical sense.

DR. SIRY: It's not closed, I guess, in that sense either, plus the fact that the node regresses.

DR. ROMAN: I guess what I was really thinking about was in connection with Dr. Dicke's experiment where you have to determine the period and the perigee accurately. Can you weed out the other effects sufficiently that you could hand knit something that you could call period in the position of perigee?

DR. SIRY: Well, yes, that can be done. This matter of weeding out the effects, of course, is the heart of the problem, but I don't think the fact that the orbit is not closed causes particular difficulty. So that the kind of uncertainty we are talking about here is of the order of 100 seconds of arc for the radio observations and for the so-called field reduced Baker-Nunn operations. These are the reductions that are made as you would expect from the terminology in the field. Now there is, of course, one other aspect that enters into this figure of 100 seconds and that is the theory since, of course, in a differential correction one compares the theory with the observations. The theory that we are talking about here is usually one that assumes that the atmosphere density does not vary with time, and it considers higher harmonics up to, say, the fourth harmonic, while the initial theory just included two harmonics. The theories were roughly the same in both cases. In other words, theories used either to reduce radio operations or the optical observations generally tended to involve this kind of set of assumptions. This all leads to uncertainties in position for let's say a nominal height of a thousand kilometers of the order of a half a kilometer (writes on board). And it's the same in this case. Now the uncertainty in velocity is another question because as far as these observations are concerned, the uncertainty is extremely small. You can see that point easily if one considers an orbit for over an interval of a week. It's a total motion of the order of some millions of miles, and with the uncertainty of this order of position, the uncertainty of the velocity from that standpoint is negligible. But one does, of course, take account of the fact that the radius of the earth is known with a precision of somewhere between one part in 60,000 and say one part in half a million, somewhere in that range. This then leads to an uncertainty in the velocity component of something of the order of 10 cms per second, but the point is here that this is a number that's associated not only with satellite observations and theory _per se_ but also with matters having to do with the radius of the earth. The next thing that's being done is to improve the theory. By that I mean the theory that's actually in the computing machine. The only theory that was available when satellites were launched that could take into account any reasonable number of effects those due to oblateness, drag, etc., was well I don't know that you would even call it a theory, but the only technique that was available was the technique involving numerical integration. With the machines that were available at that time, and the differential correction techniques that were available at that time, the process was not very efficient. So as a result Pergut and Mussen developed the modification of the Hansen's theory which was one of the ones used and there were a number of closed forms or series which were used in getting these kind of results. Now one of the difficulties arose because of the fact that it was not a simple matter in all cases to get additional effects but additional effects have been added. In particular, we have a third harmonic and some Tesseral harmonics and a little more sophistication as far as the atmosphere is concerned. In other words, the density here is not taken as a constant; the derivative held fixed is pushed out to the second or sometimes even the third. With refinements of this kind in the theory, it appears now that one can get down to something of the order of 10 seconds of arc. The Smithsonian people have been pushing ahead in this direction and getting the actual precise observations to the point where they could be put into the machine in reasonable quantities and are just now starting to get results of this nature. Now, of course, this implies that the uncertainties in the position are of the order of 50 meters. Here again we have to put a bracket around it in exactly the same sense that we put a bracket around the velocity uncertainty. The point being that we are down to a number that is of the same order as the uncertainty in the earth's radius. The limiting factor would be the uncertainty in the earth's radius. There is some hope that new instruments can be actually gotten to the point where they can be used in connection with actual satellites, and the additional techniques are the ones involving radar and Doppler measurements; in other words, there will be measures of range and range rate. The net effect of the addition of these new kinds of measures presumably will be, roughly speaking, to let us achieve accuracies of these orders. In other words, it will hopefully be possible to measure things directly with the uncertainties of this order and not have to deduce these qunatities from the theory on account of the fact that the uncertainties in the earth's radius have limited us. The point, of course, is that with the angular measures we don't measure position directly; these are only inferred by the theory and by these constants. When range is measured directly with the uncertainty of 50 meters then, this will be an observational uncertainty - not one that has these devious connections with the theory. And presumably it should be possible to measure velocity components relative to the station with an accuracy of that order and perhaps even of a better order. There will be further work in connection with the theory. In other words, in say letting the atmosphere vary with time, actually it's planned to consider the atmosphere, not only as a function of height as it's always been considered, but also as a function of longitude, latitude, and time in a more complicated way than is being done at present. The point here is to try to represent the atmosphere in the way it appears to be at the present time. There appears to be, as you probably know, a bulge, roughly speaking, under the sun, and by a bulge we mean a set of contours, lines of constant density that rise in the usual region near the sub-solar point. The axis of the bulge is presumed to be displaced by about 2 hours toward the afternoon, so that in other words the upper atmosphere behaves in approximately the same fashion as the lower atmosphere. The increase in density here is of the order of a factor of 3 at a given height and up at heights of say 600 or a thousand kms the increase may be a whole factor of 10 at a given height. In other words, the density at say 1000 kms at the bulge axis can be perhaps an order of magnitude greater than the density around the bulge axis projected toward the night time side. Now with the inclusion of more sophisticated models of this type in the theory, it should be possible to actually realize all the potential accuracy that one has in the Baker-Nunn operations later on in measures of range and range rate. Then, of course, the more that is learned about the earth's field the more harmonics will be put in, so that in the future, we'll have better observations and a better theory and will get down to uncertainties, as I say, of these orders. You can see that one has a problem here for satellites of low altitudes and even of moderate altitudes - in particular, in connection with this experiment that was just referred to. The hope was to measure the change in the period as the earth moves around the sun, and the hope was to detect two parts in the 10^{11}. The period, of course, is changed by virtue of the drag effect. Roughly speaking, the rate of change is proportional to some kind of integral of the density but we can for the moment consider that this is roughly proportional to the density near perigee. You can see if the density near perigee varies with time, as it does, then the rate of change of period varies with time. It has been learned that actually the density in the bulge exhibits a rather remarkable correlation with the sun-spot cycle. If one plots the observed period decrement which can be interpreted in terms of an atmospheric density at some reference level, the results look something like this when the period is about 27 days. So there is definite evidence that the density varies in accordance with solar activity and it also varies occasionally because of the flares and that sort of thing. Not all types of flares give rise to increased densities at satellite altitudes but some types of solar activity do. So, in other words, any complete definitive theory of satellite motion has to take into account somehow the state of the atmosphere, and this is of course in general observed only directly by the satellite itself via this change in the period. One, of course, can infer that this is due to solar activity. In effect, one is actually studying the atmosphere every time he determines an orbit. One of the difficulties lies in the fact that the period decrement even for a satellite at a high altitude on the order of say 1000 kms is not negligible, and unfortunately, it's going to be many powers of 10 larger than this 2×10^{-11}.

PROF. POUND: What would happen if you went up higher?

DR. SIRY: Well the difficulty is that no one really knows exactly how density really falls off as a function of altitude. If you plot density versus height you get the usual log rule of the density versus height, but as you get to the high-temperature readings, the decrement decreases, and after while you go from the usual low-level mixtures to atomic oxygen, and at some point you go to hydrogen, and when you go to hydrogen the scale height jumps by almost an order of magnitude so that effect versus scale height can be up over a thousand kilometers. One would have to go up many kilometers, to be sure, and then of course there is the matter of electrodynamic drag. It's not obvious that the drag is all due to neutral particles. This whole question of exactly what the drag effect is due to at heights of a thousand miles or so is still in the status of almost a research problem.

PROF. SHERWIN: Could I interject a question about the compensation for the drag by using the concept of a free-falling mass shielded from drag effects enslaved by an outside satellite with gas of some kind? It also shields it from certain types of magnetic effects. Could this, in principle, remove the drag?

DR. SIRY: You mean float one satellite inside another, is that what you say? Well, that's of course the falling sphere experiment. This is done with rockets. These falling spheres are ejected from rockets and they work essentially that way. There, of course, they are limited by the integration time. It only falls for a few minutes they don't do any better than one does with Pirani gages and things like that.

QUESTION: Is the concept a trick of structure?

DR. SIRY: Yes, with some tolerances, and the point is at a certain level of dynamic pressure and accelerations, you start to get your readings.

QUESTION: There is a difference though of a falling object when the shell is recentered by some impulse relative to the object inside. Isn't that right?

DR. SIRY: Well, yes, the thing is reset, etc.

AUDIENCE: I think what he is speaking of is the resetting would have to be by an impulse from the outside so that you don't touch the internal satellite.

DR. SIRY: Is that any more necessary in this case I wonder. Oh, you mean you want to jockey this thing around.

PROF. SHERWIN: Well it has the feature, you see, that the readings of the engine that has been telemetered down give you a precision record of these fluctuations in the atmosphere, and it gives you a satellite of true gravitational structure that has no drag effects whatever. The only question is, is it economical to build?

DR. SIRY: Well, you see the point is you would have to have a servomechanism to do this jockeying and this wouldn't be a simple system, I'm afraid.

PROF. SHERWIN: These are very tiny thrusts?

DR. SIRY: That's true but you need sensors . . .

PROF. DICKE: It would be no more difficult than the things we build in the laboratory every day.

DR. SIRY: That's true, but it has to operate up there. Most of the things that go into orbit are extremely simple compared to the ones that are built in the laboratory every day, but you have to get them through the 10 g's in 4 minutes.

PROF. DICKE: What does it cost to put something up at all?

DR. SIRY: Two or three million dollars. The satellite itself costs a million, and the launching costs a million or two depending on the rocket.

PROF. DICKE: Excluding the cost of instrumentation, I think we could guarantee to do this for 10 percent, $100,000.

DR. SIRY: Well, what you are proposing is a method for measuring densities at satellite altitudes.

DR. ROMAN: I don't think so, Joe. I think basically this is a method for getting rid of the drag. I happen to feel that this is probably the way this job is going to have to be done.

DR. SIRY: But you see this is in a sense a semantic problem because the way I phrased it, this is a method for measuring densities at high altitudes, and, of course, that's in a sense exactly equivalent to what you are talking about.

DR. ROMAN: Because we could also get rid of radiation pressure at the same time.

PROF. SHERWIN: And electromagnetic pressures, not necessarily from torques.

MR. JONES: Also separate out the gravitational effects. Or does it go on the assumption that gravitational effects are not shielded, but other things are, and then this really separates them or not.

PROF. THOMAS: Are you proposing to put a rocket motor on the satellite to keep the internal object centered?

DR. SIRY: Let's imagine what would actually happen, you have here neutral electrodynamic radiation pressure and whatnot. Now, of course, these forces are negligible and, of course, the countering forces have to be of the same order. And you get these with an electronic beam essentially or with an ion rocket. An ion engine is the only way to do it that is practical.

PROF. SHERWIN: You don't even have to stabilize the satellite because all you have to do is to know which direction the drag is occurring so you merely modulate the engines. You modulate in such a way that you get a net thrust to balance it out, an exciter which doesn't ever have to be stabilized.

PROF. DICKE: There would be considerable advantage to letting it spin too because it would keep changing the orientation of the outer relative to the inner, so there wouldn't be any systematic troubles tinkering with the work functions.

DR. SIRY: Of course, there are other ways presumably of trying to measure densities I realize. Of course, here you measure other things. The point would be to do both but you can use presumably Pirani gage techniques and try and extend them.

PROF. SHERWIN: One nice thing too is that whenever something goes wrong you can record that so that you know something went wrong. As long as you don't get any contact between inner and outer shell in that period of time the orbit is absolutely guaranteed gravitational only.

DR. SIRY: Right. But you see, to do this, first of all you have to get an electrostatic engine into orbit and that of course hasn't been done yet and probably won't be for a year or two or maybe more. To make that work at these thrusts and to have the proper dynamic range and to have the proper servomechanism to sense the changes, etc., this is not going to be a simple experiment.

PROF. DICKE: What do you mean by electric engine?

DR. SIRY: Oh, electrostatic propulsion is presumably the kind of thing we are talking about . . .

PROF. DICKE: Why not gas jets, it takes very little . . .

DR. SIRY: Well, you want to get something in the same order as the densities that are up there and you are talking about pretty small pin holes. Even with vacuums, the kind we talked about inside here, you must maintain the kind of vacuum that they have in the accelerator in order to get a flux that's of that order. This isn't simple. I'm not sure that these techniques are developed to the point where you could do it in two or three years.

QUESTION: What is the drag force?

DR. SIRY: It's a small force; it's negligible compared to the drag at most altitudes for ordinary satellites. The radiation period.

PROF. DICKE: What is the drag or magnitude of say a thousand kilometers in dynes?

DR. SIRY: Oh, the deceleration is of the order of 10^{-5} or say 10^{-3} minutes per day. So this would be around an orbit you would get a centimeter per second per day or 10th of a centimeter per second per orbit. This is 10,000 seconds so this is 10^{-4} cms per second per second - something of this order, 10^{-7} g's, or somewhere in that region.

QUESTION: Let see, the air drag is not the same order of magnitude as the radiation drag. I don't remember that.

DR. SIRY: Oh, it's 4 or 5 times 10^{-5} but it's ~~g~~ dynes per square centimeter. Yes, 10^{-5} per second for the radiation pressure.

PROF. DICKE: It should be very easy to correct such tiny forces with small jets.

DR. SIRY: Well is there anyone here who has worked with vacuum techniques who would comment on this. (Everyone talks) I know but you have to be able to achieve those kinds of forces on a reproducible basis.

AUDIENCE: (All talk at once. Mention servocontrols.)

MR. JONES: It's a problem that ought to be studied.

DR. SIRY: Well I wouldn't say it has been studied. The falling sphere people have looked into these things, I don't know to what extent, or how recently.

DR. ROMAN: The falling sphere is somewhat different though.

AUDIENCE: The falling sphere simply recenters the ball inside the cabinet.

DR. SIRY: These are also the people who work with the Pirani gages and pinholes and this sort of thing and have some feeling for the way these things get corroded. You see you don't know what the ambient conditions are really and you are talking about a system you can get operating in a laboratory. You have a servo system and one of the constants is pinhole size, you have to have reasonable dynamic range in the electronics to take care of the reasonable changes of pinhole size. What's reasonable? 5 powers of 10? Suppose your dynamic range doesn't cover the change of the pinhole size?

CHAIRMAN: I don't think this is the place to design this thing.

PROF. DICKE: I can design this by magic if we continue talking.

AUDIENCE: May I make a comment about the semantics of the situation. It depends on your point of view whether you considered this device a way of measuring the density of the atmosphere or whether you consider it a way of not caring what the density of the atmosphere is for some experiments for which this is computed to be predominant.

DR. SIRY: Well, that's true. Of course, one would be the by-product and one would be the primary product depending upon your interest. But the point I'm trying to make is that all of these ideas that have been proposed are obviously technically feasible but I think in most cases they are an order of magnitude more complicated than the kinds of things that are actually flying.

CHAIRMAN: Suppose we assume that it can be done, then what?

PROF. DICKE: Grant us the $300,000.

DR. SIRY: I don't think you would ever make it with $300,000; the environment tests alone would be much more than that.

AUDIENCE: What is the rate of change now due to the drag?

DR. SIRY: It runs from 10^{-3} to 10^{-5} minutes per day.

PROF. NORDSIECK: What is it in the same units?

DR. SIRY: This 10^{-11}, well it's about 3 or 4 powers of 10 larger. The difficulty is that you can see the problem here. This effect is hidden.

CHAIRMAN: What accuracy can you expect arising from the theory with the knowledge of the gravitational field? That's where your problem is.

DR. SIRY: Well what you are introducing is from one point of view another type of observation for which it would be very interesting to compute a differential correction. (I just mentioned it, of course) As I said, these things can be done but we have yet to put on a scientific satellite the simplest kind of pressure gage you can imagine, or density gage or a gage to do, in effect, the kinds of things we are talking about. These things are not yet flying in their simplest imaginable forms. (I was using "we" in the restricted sense*). It's not clear what they got from these gages as a matter of fact, but at any rate I say on our side we haven't actually put anything to work yet that's an order of magnitude simpler than the kind of thing you are talking about. I think obviously this kind of thing should be done but from the standpoint of what the state of the art can now achieve. I think that's a half generation downstream. But obviously it's a very intriguing suggestion.

PROF. SHERWIN: One reason it's particularly interesting is that if you ever make a free gyro, really to protect it too you develop techniques to make it spin. And now you track a star or something. You've got a gyro that doesn't have any differential light pressure, differential drag, or anything else that will upset precessions that you can think of. This is a very simple case. The next case is to put the gyro in.

DR.RROMAN: I want to come back to the question that Prof. Robertson asked. What problems, if any, does the effect of the earth's gravitational field and not that of sphere introduce in this problem?

DR. SIRY: You just have to give more terms to the theory, and they are somewhere in the 3rd or 5th harmonic and also one term or maybe two in the ellipticity of the equator. These terms let you get down to 10 seconds of arc - maybe even 6 seconds of arc. The point is though, in effect, you can only go as far as the

*Excluding the Russians.

observational accuracy will allow you to and we are probably not too far from the end of the line now as far as those kinds of things are concerned.

CHAIRMAN: Does that give you any hope of determining the effects that Dicke is talking about?

PROF. DICKE: With the kind of tracking scheme he was talking about, there is a question.

PROF. SCHIFF: I could quote one number and that is if you take a satellite in the equatorial plane at moderate altitude and you take the commonly quoted figure for the difference between polar equatorial radii here somewhere between the homogeneous assumption, it turns out that the precession of the perihelion direction is about a million times bigger due to the earth's bulge than it is due to general relativity.

PROF. DICKE: What altitudes?

PROF. SCHIFF: Moderate altitudes, 500 to 2000 kms. It gets relatively small at those higher altitudes and the general relativity effect falls off like $1/r$, and the bulge effect like $1/r^2$ so to separate the general relativity effect from the bulginess effect with the satellite seems to be very difficult. It has to be of a very high accuracy.

DR. SIRY: Well it's on the order of a fraction of a meter per day, the motion due to the bulges are a few hundred miles or a million or a million and a half feet per day, about a factor in 10^6, so this is about a foot or two per day. You probably couldn't see it until it got up to a thousand days worth.

DR. ROMAN: You do have a value for this effect. You know something about the harmonics of the earth to start with, for most satellites, so you can take out some of this, but you can also go up to 10,000 kms if you want. If you go to 10,000 kms what effect is left uncertain? Are you uncertain of the value of the harmonics of the earth's field? Have you any idea?

DR. SIRY: Well, let's look at it this way. You are talking about something that 10^{-6} of the oblateness effect, or roughly speaking 10^{-6}, say this constant or this one plus this, etc., and of course you don't know these constants that well. They are small to start with, you see. Let's assume it gives rise to a motion of a foot or two per day in the motion of perigee. You have to integrate this over a thousand days before you get it up to the size you can see. This is three years. Or to state it in the other terms, you have to be able to measure this effect to one part in a million and I think that's a little beyond the present state of the art. As I say, all these things are perhaps in a little more elementary state in practice than they are in usual discussions.

PROF. SCHIFF: There is another effect that Prof. Little remarked on earlier and that is if you are not careful when you have the kind of thing that Dr. Sherwin was talking about with a shielded satellite i.e., with the outside slave of the inside. Residual forces may be exerted on the inside one by the outside and these could be gravitational for example if it's not a homogeneous mass shell, and off center; or they could be electrostatic or magnetic. This would sort of be the dog chasing it's own tail.

PROF. DICKE: This would be one advantage of having the outside shell spin.

DR. SIRY: This, of course, is typical of exactly the kinds of problems people run into when they tried to make measurements of these kinds in rockets and satellites. When the first measurements with the ion traps were made 15 years ago on V2's, it was many many years before they could be interpreted properly. It is not even clear yet that they are being interpreted properly as of today. And of course you have to be able to do that to run the experiment.

AUDIENCE: In the acceleration of the sphere in free fall, you have to shift around about the sphere. The acceleration is of the order of 10^{-11} g and I've been trying to track the center of the mass of the shift. It is a very small effect.

DR. SIRY: As time goes by one will know more about the density at high altitudes. Right now it's really a matter of speculation to estimate what the drag effect would be at 2000 miles, or at 3 or 5, because one doesn't know the scale. One doesn't know whether the oxygen is predominant, or hydrogen, or atomic oxygen, or whether the drag effect is due to charged particles due to the plasma effect.

PROF. SHERWIN: Are certain orbits better than others for measuring the precision of the plane? Is an orbit in equatorial plane say at a thousand miles a pretty high symmetrical situation? Wouldn't that be quite insensitive to these constants?

DR. SIRY: Well you see you have to achieve symmetry with respect to this bulge whose axis is 2 hours displaced from the line of the sun, of course the axis moves up and down between the tropics and that's a little difficult. You see if you put it on the equator, the bulge would run up and down through it.

PROF. SHERWIN: I'm assuming that you don't have your machines and you don't have any drag problem and you are limited now by the uncertainties of the gravitational field which of course would be greatly reduced if you could remove drag. You would be forced back by limitations in determining position.

AUDIENCE: I don't think you can determine the even order harmonics with the equatorial orbit very well.

AUDIENCE: That's what you want to know, you want to see if a satellite is slowing down.

AUDIENCE: Sorry, I thought you were suggesting that he determine those constants better that way.

PROF. SHERWIN: You put it in polar orbit, the drag will make it possible to make a much higher precision determination.

AUDIENCE: Right. The present limitations are set as much by the drag as they are by . . .

DR. SIRY: Now of course, it might turn out that a satellite at 5000 miles altitude is negligibly affected by the drag, there is no way to really assert that at the present time on the basis of what we know now about the atmosphere.

AUDIENCE: MacDonald wrote a long article about constants . . .

DR. SIRY: Those constants sometimes appear to be in the same state as some of the other ones we have talked about. Of course you know the history of how this problem is done. One day it is completely negligible. For awhile they thought it was dominant. I'll just mention some of the other effects that one has to consider and these are of course the sun's and the moon's gravitational effect and the radiation pressure effect even on an ordinary dense satellite. We are all familiar with the fact that the perigee of Echo went down several hundred miles due to radiation pressure; but even the perigee of Vanguard moved an appreciable number of miles in over a year and you can split this up into the gravitational component and radiation component. They are approximately the same order of magnitude. In any experiment that you can propose, you would have to have pretty good understanding of these effects and in particular the radiation pressure.

PROF. DICKE: Not if you put a shield around it.

DR. SIRY: Leave all your problems to the ingenious experimentalists.

CHAIRMAN: You would have to guarantee that the shield would work.

DR. SIRY: Yes, that's where you can solve all the problems by the assumption (interruption), of course your shield wouldn't take care of your gravitation perturbations would it? I think if you went down through the extra powers of 10 you would probably uncover other effects that might as usual cause difficulty, but it would certainly be an interesting road to pursue. It's true that things more complicated than this have been flown, obviously; but these are not the kind that cost a million a piece. These are the programs that run into the tens and hundreds of millions, we need to get a really elaborate servo system in orbit that will work over a long enough period of time. And this is another minor detail, you see Tiros, which is in principle by these standards a trivially simple thing - it's just a TV camera and lens, solar power supply, and there are hundreds of thousands of these that work on ground for many years. It's not a new instrument. As a matter of fact, the first Tiros contract called for 90 days operation in orbit and it quit on the 90th day. They did a pretty good engineering job. The point is when something is relatively simple, it operates for only 90 days, and you would need operation over a year.

PROF. DICKE: Prof. Pound and I would not agree that a television set is simple.

AUDIENCE: It is much easier to make a gas valve than a TV set.

CHAIRMAN: We didn't solve it the first time we tried it and I don't think we will this time. Could I ask about this observation? To what extent is there a requirement for optical observations with the use of, oh say, Schmidt telescopes? I don't know what one would get in addition to the Baker-Nunn system.

DR. SIRY: Well this is a Super-Schmidt system that's where the whole design came from. Whipple had his Super-Schmidts in . . .

CHAIRMAN: How large are they?

DR. SIRY: About so.

DR. ROMAN: About 24x36.

CHAIRMAN: So that additional observations wouldn't add particularly to the solution of the problem.

DR. SIRY: No, you see you're down nearly to the bottom here. You have a plate and the images and you have the astronomical technique not quite to the full precision for various reasons; timing is, of course, one of them. There are ways to get around that. Of course, I should mention the whole range of ideas that the geodetic satellite people have come up with - you know, flashing lights, and photocells on the ground to get the time or else a telemetering system to telemeter the flash time so that you eliminate this problem. There are higher frequencies you can go to, and you can go to angular measures and Doppler systems, etc. None of these has the interesting property that they just bypass the whole thing the way this does.

PROF. DICKE: What sort of optical accuracy do the people have with field conditions?

DR. SIRY: Well, you know what the astronomical figures are. They are a good deal better than that. You see you talk about field conditions. This is not Palomar. This is a Baker-Nunn station in South Africa and India where I guess even water is a problem, and I'd say they do reasonably well.

PROF. DICKE: What's the angle accuracy you get optically?

DR. SIRY: Within several seconds of arc.

PROF. DICKE: You ought to be able to get a few tenths.

DR. SIRY: You see this is a matter of the writing speed this is not a matter of a half hour to get an image. You know the satellite moves by and you have a moving camera system. So you have a different kind of mechanical problem.

PROF. DICKE: This is one of the parts of the problem we will take though, this problem of precision measure, either with a flashing light or a reflector and a searchlight. You track the stars the way you normally do with precision measurements. It looks like you ought to be able to approach the accuracy that you get with more usual star precision. There is about a factor of 10 to be picked up there.

DR. SIRY: Yes, I'd say that's reasonable. Of course you know the geodetic satellite has been a reasonably active proposal for a couple of years, but it always manages to lose out on the budget provisions. There are always so many things more interesting that it never quite makes it.

CHAIRMAN: I had hoped that we might catch up the hour and a half but we bogged down in the design session. Is there any more discussion on this last point that anyone would like to bring up?

AUDIENCE: I might make a comment on the gravitational field problem. Keelsey and some other people have done work recently in

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determining the higher harmonics in the earth's gravitational field and the indications are that they are quite significant, that the early determinations of second and third harmonics were really average values which were based on this single satellite measurement and so on. As you get enough satellites into long-time orbits covering enough space so that from secular and long period observations you can determine more about the gravitational field, these things become more significant. Then he indicated that these things are significant up to the 9th, 10th and 11th terms and there is no reason to expect that there aren't going to be 12th, 13th, 14th and so on. So that even if you have the measurements and you can actually determine a certain number of these coefficients, you are eventually going to get to the point where you've got to decide, OK, that this is enough on the gravitational field; now to separate out the other effects, the ones you are looking for. How far do you have to go in determining the gravitational field? I don't know, but since almost all the determination that has gone beyond the second harmonic is based on satellite observations, you are stuck with the problem that you will never be able to distinguish two things, both of which are based on satellite observation if there isn't some weighting factor or something else that will distinguish higher harmonics.

DR. ROMAN: Am I right in thinking that with the higher harmonics you go down in importance rather rapidly with altitude?

AUDIENCE: Yes, but if you have two different altitudes in which you have your satellite, you still can only determine, even for a fixed inclination angle, essentially 2 harmonics.

DR. ROMAN: Yes, but if you go higher, the 9th and 10th harmonics aren't going to cause you any trouble anyway.

AUDIENCE: Well it goes down with $1/r^{n+1}$. . .

AUDIENCE: But you need enough measurements, you need enough satellites at these various altitudes in order to be able to make a distinction between these.

AUDIENCE: Well these effects in a sense are not on the period of the first order, so you can carry out this period experiment to the second order.

AUDIENCE: You would have to consider both ways.

ADJOURNMENT: 5:00 p.m. Thursday, July 20, 1961.

Second day of Conference on Experimental Tests of Theories of Relativity - July 21, 1961, 9:00 a.m.

CHAIRMAN: I will call on Mr. Mitchell who will talk on both aspects according to Hall and himself.

MR. MITCHELL: Well everyone else starts off by writing equations on the board. Just to show you my background so you'll know there is a spy in the house I'll start off with Murphy's law, the famous engineering law, that if the probability of A is greater than 0, the probability of A must be 1. This is sometimes called Murphy's law and this comes in quite naturally in all aspects of satellites. (writes equation) I would like to give you a general picture from the practical aspect of what it takes to get a satellite into orbit. And I'll say just a little bit in general of vehicle capabilities, spacecraft capabilities, discuss some of the problems of environmental testing, experiment design factors, and finally say a word or two about what you might not think is an important thing, but which is the question of management and the actual running of the project.

<u>Vehicle Capabilities</u>: Now I purposely will make these in very general terms because I don't want you to go right out and design experiments on the basis of these, but you have to know what ball park you are in and that's the purpose of these remarks. In the first place what vehicles are available? We start off with the smallest one, the Scout. If you have anything much bigger than 150 lb for a low orbit, you can forget this vehicle. If it's much bigger in any dimension than 2-1/2 feet, you can forget it for that particular vehicle. The other thing about the Scout, the other pertinent factor, is the maximum acceleration your equipment would see is of the order of 30 g. That's the Scout, the smallest vehicle we have with an orbital capability. Now next is the delta configuration and this is used quite extensively for a number of scientific missions and is very successful. 500 lb is a number for a low orbit; I'll put another number on here just to give you an idea. With the delta you can probably have an escape payload of 50 lb; that would be the capability for an escape mission or an equivalent mission. Maximum accelerations are a little lower, 12 g. In size this spacecraft is about the same as the Scout, maybe a little larger. A typical dimension is the order of 3 feet (interruption; centimeters?). That's alright, I can translate back and forth. Now that's why I put this up here, so I'll be talking generally in feet. I'm a sort of schizophrenic engineer now that I'm associating with scientists, and I don't know whether to say feet or centimeters. Now we have two other configurations the Thor and Atlas-Agena. The Agena stage is the second stage. The load with the Thor configuration is about

1500 lb, and with the Atlas configuration about 5000 lb. Thor-Agena has no escape capability for reasons which I won't go into here. The Atlas-Agena escape capability is the order of 700 lb. On the Thor the typical dimension is 5 feet and on the Atlas-Agena, 10 feet. 10 feet is the order of magnitude of the orbiting astronomical observatory mentioned; the accelerations are much lower, about 7 g. Next in line is the Atlas-Centaur, a configuration with an Atlas booster and a liquid hydrogen upper stage. The capability there is about 8000 lb, and something of the order of 2500 lb for escape. There again a typical dimension is about 10 feet; the diameter of the Atlas is 10 feet. The maximum accelerations are about the same. Just a word about the low orbit; these numbers are representative of an orbit with an eastward launch from AMR. If you want to talk about polar orbits for instance just to give you an idea of the numbers on the perigee and on the polar orbit the engineering number is about 900 lb. If you want higher inclinations or you want to do tricks with an equatorial orbit the only place we've got to launch from is AMR, so you have to fly down and do a dog leg maneuver and this costs you in performance. One more set of numbers that I will put on here, are for the so-called Saturn C1. I won't go into all the configurations that are being looked at for the manned mission. There are a number of these. Nova and Saturn C2's and C3's, etc. The Saturn C1 is the first thing that will be flying and this has a capability of about 20,000 lb. The number I have here is about 56,000 (escape speed). A typical dimension is perhaps 15 feet and the accelerations I think are the same (7 g). Going on to other configurations beyond the Saturn, configurations that would have larger liquid hydrogen stages, and would make use of the F1 engine (the F1 engine is the single chamber, million-pound thrust engine), you can start talking about numbers of 40,000, 50,000, a 100,000 lb (you have to for the Apollo). We are talking about a time period of 67. Others are avialable right now. First the Thor-Agena is essentially available and the Atlas-Agena has been flown in the military program; the Centaur has not yet had the first flight.

QUESTION: These are net payloads?

MR. MITCHELL: These are net payloads and I will emphasize that I put these up here for orbital ability. If the vehicle people come back and you tell them that Mitchell said I could have an 8000 lb payload and here it is 8000 lb on the Centaur, I will deny it. I do want to emphasize a number of things, but these are the orders that you need to work with when you are in the conceptual stage. So much for the orbit capabilities. How about spacecraft capabilities? Well, I could get into a lot of detail on that but I think I will just summarize some broad numbers which to me are significant. At the present time if you consider first structures and then power supply and stabilization and control, (these are all engineering sub-systems) then you look at telemetry systems, at thermocontrol, data storage, sensors, (I'll put it down in this form) and then look at ground support. Let us ask ourselves what order of magnitude from an engineering standpoint is possible now, and where can we expect to be in the next 4 or 5 years in these areas. Structures I will give in terms of weight, 10^2 lb. The next generation things are in the design phase now. In the case of the orbiting astronomical observatory, you are talking of 10^3 lb, say 2,000, 3,000, 4,000 lb, within the capabilities of the Agena and the Centaur. Power supply, power supplies for typical scientific missions, tens of watts. I am speaking of the solar power supply, based on solar cell efficiencies of 10 to 12 percent. 12 percent efficiency is just now coming into use in a practical engineering sense. The power supply on the orbital astronomical observatory is I think somewhere in the order of 350 watts or in that ball park. Of that 350 watts about 30 watts are available to the experimenter. The rest of it goes to the stabilization and control system, data storage, etc., etc., just the laboratory in other words.

Stabilization and Control: In spite of a number of very successful missions with three-axis control, such as the Russian photographing of the backside of the moon, some stabilization on the Discoverer series and Air Force series, essentially one-axis and spin-stabilized control (Tiros) is still generally the thing that you can rely on for weeks and months. All these other things are for days. The control is effectively single axis; I'd say it's essentially one and one half axis because you do have some sort of control - you can precess the axis a little bit on command. Now the much more complicated things we can go into will be of course, three axis, and here what sort of accuracy is generated for long time is an accuracy of degrees, or even 10's of degrees, for spin stabilization. The orbital astronomical observatory as now proposed uses a three-axis control system with a course accuracy of one minute of arc, a fine accuracy, hopefully, maybe, of a 10th of a second of arc, for some of the experiments that are involved now. This is an experiment that Princeton expects to get with the third spacecraft.

DR. ROMAN: You might mention though that in order to get this accuracy you have to have a telescope of 30 to 40 inches on board so that you have an error margin. You are not going to plan on a few tenths of a second to ride piggy-back on another experiment.

MR. MITCHELL: I would like to emphasize that that one minute of arc is the basic accuracy expected in the laboratory of spacecraft itself. Then after you have your experiment in the laboratory you've got to come along and find an error signal to get the tenth second of arc.

Telemetry: I want to include a lot of things under this telemetry. Right now there is generally the single link, and there are exceptions to this. Tiros has a couple of links, a wide band wave and a narrow band wave. The frequencies generally are 100 megacycles or so at this range of frequencies. Ability to command this telemetry or of the whole data system - here you have the possibility of about 10 different commands. This is characteristic of a small satellite. The trend in that is to multilink systems, that is, telemetry systems, both narrow band systems and wide band systems, and tracking beacons, so that on the OAO and the orbiting geophysical observatory there are three or four different telemetry links. Usually these are double so that there may be six or eight effective links that you can get the data from. Well the other thing that is extremely important is the fact that here we have a command capability of ten commands. The command capability that's now being designed in these things gives you the capability of 10^2 commands; for instance, I think of the order of 250 different commands. This is a digital command system that's used if you put in the appropriate switches, the appropriate circuitry in the satellite. Then you can sit on the ground and you can look at the readings, ask it to turn on this switch, turn off that switch, this sort of thing, up to several hundred.

DR. ROMAN: You might make the qualification that you made earlier about the power. The majority of these commands are going to go into spacecraft operation.

MR. MITCHELL: Yes.

Thermal Control: Generally thermal control is still being handled by passive methods. Actually on the more complicated vehicles there is nothing more complicated in the way of thermal control other than some little movable disks, in other words, there have been no systems yet that have been actively worked on where you have a liquid coolant or some more complicated system. The passive systems have worked our reasonably well. Of course it gets more complicated as you install more power and the power density perhaps goes up. You have an internal heat dissipation in a problem associated with giving the various sections of your laboratory the temperature that you want, but as far as the method of handling it by controlling the absorptivity and emissivity, this is the general procedure that's being followed.

Data Storage: The capabilities now, with proven equipment in the satellite are 10^5 bits data storage, 10^5 bits on magnetic tape, and in some cases small core storages. We have a requirement and this is going to be a bit difficult to obtain on the orbiting geophysical observatory 43×10^6 bits for one tape recorder. This is what we would like to have so that we could record the data for 8 hours at a low rate. This is in duplicate; there are two of these so you could put the data in parallel. Thus using this as a backup, in other words, you could use the total capability. 10^8 bits is going to be difficult to get in a reliable sense. Another data storage system being developed is a rather interesting one for a lot of applications, for the orbiting astronomical observatory has a core storage. People that use core storages and computers on the ground want to have an air-going core storage such as that being developed for the orbiting astronomical observatory, where the storage is 10^5 bits and is doubled. It is a core storage, with random access, 1×10^5 and you can double these up, as a matter of fact we are doubling them up. When I say sensors here, I'm just going to give a number. If you look at the typical small satellite you may find as many as 50 different sensors and these may vary from simple temperature measuring elements that are performing throughout the spacecraft to the actual measurements that are being taken into the spacecraft itself, the scientific measurements voltages, curves, etc., geiger counters and various things. These get more complicated as you go up here. This gets to be 5×10^2 and it's disturbing to me that out of that 5×10^2 I think you have to take about 4 of those just to make measurements to see whether your gyros are running, your power supply is running right, all of your telemetry system, so that you can go into these alternate modes; these are the housekeeping functions. Now the data rates, I'll admit, are very low, one minute or one second or so in this type of measurement. But that's the sort of thing you can picture with the more complicated spacecraft. A large number of things just to note the performance. With something in the laboratory you can look and see how it is performing, but when it's away from you, you have to put some instrumentation in it. Now generally you can get by with Minitrack. We have seen no requirements for any of the scientific missions up to now; the one we are talking about today may indicate more than that. But the Minitrack is not accurate enough for the orbital determination, although it is accurate enough to locate yourself in space. These systems all require specialized ground equipment - the astronomical observatory will require specialized ground equipment; the geophysical observatory will require specialized ground equipment. Some of this is being put in as part of the net, a number of large 85-foot dishes, in order to capture as much of the power as you can. In a broad sense, you can see here we are now, and here's the things that are on paper. For instance, the things that will be flown in a year and a half, two years, three years. There's an order of magnitude increase in an engineering sense, the structure is larger by an order of magnitude, the power by an order of magnitude. The control problem may be more than an order of magnitude; data storage is varying, so generally we could sum it and say: the sort of things that are on the design board are an order of magnitude more complicated. This is fine to say you have that capability, but it is another thing if you have a nice pretty instrument. You will say I've done this experiment before, but how would you like to do that with your Hamilton (at this point Mr. Mitchell takes his watch from his pocket and drops it on the table) or something else. This is what you have to do with your instrumentation. In effect, it has to stand up to 30 g if you are going to fly it in a Scout or 7 g and you calculate in some factor of 2 depending on how you feel about this sort of thing. So your nice pretty instrumentation that is easy to do in the laboratory (I'll just make another sort of general rule) in order to do it in a sounding rocket the effort you have to expend in terms of mourning and manpower is 10 times more than needed to do it in the laboratory. In order to do it in a satellite the total effort is 100 times so you go a quarter of a magnitude as you go from the laboratory to just a simple sounding rocket with the experiment and one order of magnitude as you go from the satellite. This is a general statement I know and perhaps there are exceptions to this but I would like you to keep it in mind. Just to give you an example (King perhaps knows about this since he's no doubt talked with Krauschaar and knows some of his experiences) Krauschaar flew a γ-ray telescope and I talked with Bill on this and he said that photomultiplier tubes were particularly a problem there. He had 8 photomultiplier tubes in his package and he had two satellites. He told me that he bought 240 photomultiplier tubes to get 16 and he wasn't happy with the 8 that were in the second package. Now this is the sort of thing that you run into. There are a number of other areas that I could mention along this same line - 240, and he tested them, shook them, dropped them and he got 16, and as an experimenter he was happy with only 8 of them that he had in his prime package, the one that's in orbit right now. Just to give you an idea there is something else about this environmental test procuedure; I'll just read you a couple of numbers for instance on the Ranger. I have not discussed anything in general about the interplanetary spacecraft (I'm talking in general about satellites), but I think the same sort of things apply, except the weights come down to escape type weights, and this is in the direction along the thrust axis. The Ranger structure is designed for 1.25×11 that would be what, 14 g, or something of that nature. Some dynamic tests were run by J.P.L. They applied actual loads from 0 to 40 cycles per second. These are 0 to peak oscillations so about 2-1/2 g sinusoidal for a flight of 8 minutes. In the range 40 to 1500 cycles per second, 2-1/4 g and this is, that would be 5 g peak to peak, this is for 9.7 minutes. These are the sort of things under which instrumentation has to stand up. Just to emphasize this I will look from the things that you have to have in this sort of thing some of the environmental test specifications for the orbiting astronomical observatory. Just to give you another example. Among other things, it has to stand up to 130 db sound level. So this is pretty severe in itself. The reliability that you would like is a 7/10 probability of the thing working for a year. This is extremely difficult, since there are a lot of modes on this thing so you can still get data out of it if it doesn't work perfectly. But one of the real sensitive items is the star trackers. I've seen numbers on that that varied from 10 percent to 40 percent. The probability of getting star trackers to work from year to year is extremely low. Normally your equipment has to be tested in a thermal vacuum environment and a sort of standardized test is exactly that put in a thermal vacuum with a low enough pressure so that you have no heat conduction problems. That is, the mean free path is low in your characteristic dimensions, so if you get 10^{-4}, or 10^{-6} as the characteristic pressure, and then cycle, you must run it at a hot temperature, the hottest temperature you expect for 7 days and the lowest temperature you expect for 7 days, and then operate it in the orbital astronomical observatory with the anticipated orbital operating temperature range for 16 days, or in other words, in a vacuum tank for 30 days with everything working. This is the sort of testing that you will have to do ahead of time. I don't want to give the idea that I'm a pessimist. I'm an optimist but I would like you to go into this with your eyes open. The other thing that you must understand then is that in the laboratory I'm my own boss. I can build the oscillator and I can build the sensor, calibrate and do everything myself. You can't do that in a satellite, you've got to depend on somebody else. Maybe you can build a rocket better than he can but you haven't got the time to do that; you have to depend on someone else. There are a lot of people involved that you have to depend on, somebody to provide the support for you in the telemetry system, the command storage, the data storage. You can't go out and build yourself the tape recorder that will record 10^8 day after day after day. Another thing, if you take a small satellite, and by a small one, I mean one of the delta payloads, for instance the so-called radiation payload, (a payload that weighs 83 lb), the instrumentation in it must cover the energy range from the Van Allen radiation to the cosmic radiation, i.e., must cover the energy range from just a few electron volts to a few billion electron volts. There are all sorts of counters in it as you can well imagine. A project such as this requires two years, not from the time you would think of it but from the time you get started. So you get in the design phase, you start designing the hard way. It is two years until you see the first one go out and then I will assign many probabilities and there is a finite probability that it won't orbit. It's very discouraging to see your payload fail after you've worked with it. You've dropped things, you've done things you didn't think you could do, and you see it take off, and you sit there and an hour later you hear rumors and it didn't make it. So where do I go from here. With that I think I will conclude and say that the other thing that is frustrating to you is scheduling, because there are so many like myself saying: Look we've got a date here and that date says Jan. 16th at 2 o'clock. I'm exaggerating here it says 1963 or something like this, before this date they say "Where is the hardware?" What do you mean it's twice as heavy we can't take it? (it's got to be) we got to start the prototype, we've got to start putting this thing together as a unit and see if it is going to work, and you get mad and say you don't understand the problems. But it's important though for the reason that I mentioned that there are so many things. The vehicle people are getting cranked up for their mission. Convair and I don't know who else are all getting cranked up for this date and they are all complaining too so this is the other sort of thing that you do have to put up with.

Scheduling: Even with all of this it's well worth the effort, and I would just ask you to keep your eyes open for this type of thing if you are interested in doing satellite experiments. Go into it with your eyes open, the rewards are great and the frustrations are great but it is worth the effort. Any questions?

AUDIENCE: What is your comment on the ambient temperature that you expect from the OAO?

MR. MITCHELL: I don't recall the numbers but the experiment package I think is -100°.

DR. ROMAN: One of them is down to that, I don't think all of them are.

MR. MITCHELL: I'm probably talking in Fahrenheit terms. -100° F for the experiment temperature. For the outside of the configuration a 100° F is not unusual so there is quite a wide variation. But on the Orbiting Astronomical Observatory the configuration is an octagonal shape for the spacecraft itself and the actual telescope is located in a hole in the center that's about 40 inches in diameter. This whole thing is like 10 feet long. The solar pad is on the side, so that it apparently is going to be relatively easy to control this temperature within quite wide limits but with strictly passive means, by radiation shields suitably placed on the inside and techniques of this nature. This apparently is not a great problem; at least the engineers can see how to do this.

CHAIRMAN: If Prof. Schiff hasn't changed his mind in the last half hour we will now listen to a paper on possible gyroscope tests.

PROF. SCHIFF: I find this very terrifying but fortunately I'm a theoretical physicist. So all I do is think up bright ideas and leave it to people like Prof. Fairbank, Prof. Little, Mr. Bol here to do something with them. I want to describe very briefly the theoretical ideas behind the proposed experiment and I'm being very brief because it's all been published. Mainly I want to give the experimental people a chance to talk about the experimental aspects of the (writes on board) I should say that Prof. Fairbank is not here now. He is in Varenna at a low temperature conference and therefore any question or any discussion of these experimental aspects will be done by persons who are not primarily as deeply involved in it as he. So you may not get the full answers to everything. Now yesterday morning Prof. Robertson gave a survey of what one can tell from the different classical experiments, you might call them, which deal essentially with the Schwarzschild form for the line element and also make use of the geodesic equations of motion for a mass point or for a light ray and I want to just refresh your memory on these. I'll write down the line element in the form that Prof. Robertson did yesterday: (Writes equations) This is the time α and β are actually equal to 1 in the isentropic form and γ is also equal to one, and this is multiplied by $dx^2 + dy^2 + dz^2$. This is the isotropic form of the Schwarzschild exterior solution and in the Einstein theory these three numbers, α, β, and γ are all equal to one. Now you can see, as Prof. Robertson indicated yesterday what it is that tells you about different terms here. The Newtonian theory tells you about α. The reason it tells you about α, and not about γ is that for a Newtonian orbit (suppose we imagine a circular orbit to make it simple) of radius r about a point mass M. We have $GM/rc^2 = v^2/c^2$. If the particle of the planet has a mass m, the force is the Newtonian gravitational constant times the product of the mass divided by the square of the distance, and this must be equal to the centrifugal force which is $m\omega^2 r$ so this tells you right away, the little m's cancel. It tells you then that this parameter GM/rc^2 is equal to v^2/c^2 for a circular orbit. So that means then (points to blackboard) that every term here is one order of magnitude smaller than the corresponding terms here. Thus the Newtonian theory tell you about α and tells you about 1, but as Prof. Robertson said yesterday you believe you know the one for special relativity so we are not concerned about that. Now if you want to go beyond the Newtonian approximation you must go beyond these terms; then these terms come in together. These are the two terms which tell you the next correction to the Newtonian theory and give you the precession of the perihelion of the orbit of the planet. Prof. Robertson wrote down the combination here yesterday. Now this is in the practical case for a massive particle which is moving with speeds small compared with c. For a light ray, of course, this quantity divided by this is the order of 1; (explains equation), so that in the case of the light ray, these two coefficients are equal and we have the correspondence 1 with the 1 and this term with this term, and so on. If you are looking at the deflection of a light ray it turns out that these two terms are the same order of magnitude and, again as was indicated yesterday, a certain combination of these, the sum of them, comes into the deflection of light by the sun. Now one of the other classical shifts, the one that Prof. Pound talked about yesterday, is the red shift. That involves only time comparison, so it involves the various times here. Well, the first-order red shift which has now been measured to about 3 or 4 percent accuracy involves this factor, hence it involves the same factor that appears in the Newtonian theory and which was also pointed out yesterday. If one wants to go beyond that, then one must include this term. Now to measure the second-order red shift would be a real test of the structure of the theory because it gets into the nonlinear term an m^2 term which otherwise can only be found from the orbit precession. To get into this there are two problems. The first is to get sufficient accuracy to pick up this term in comparison with this one. Now this term for the earth is of the order of GM/rc^2 plus r for the satellite, you might make r several times the radius of the earth but lets just say it's of the same order of magnitude. For the surface of the earth, GM/r^2 is just the surface gravitation acceleration, 980 cm per second squared, so that for the surface of the earth and for a nearby satellite GM/c^2r is the order of gr/c^2. This (points to equation) is 10^3, this is about 6×10^8, this is around 10^{21} so this turns out to be about 10^{-9}. Thus in order to pick up this term compared to this you must measure something which compared to 1 is 10^{-18}. Thus in a satellite you need an absolute accuracy of one part in 10^{-18} to detect this term. In the experiment tried by Prof. Pound the situation is worse than that because the primary term being measured here is quite a bit smaller. Since this term is measured not for the differential radius of the order of the radius of the earth but for a differential radius of the order of a hundred feet or so. This term becomes relatively again the full 10^{-9} of this term which makes it instead of 10^{-15}, I think you quoted, about 10^{-22} or 10^{-23}. That makes it a very difficult experiment on the terrestrial scale too. Even if you could do that though there would still be a second problem which is a surveying problem. You must know what the distance is and this radial coordinate, as one knows from general relativity theory, is a scale coordinate which must be interpreted in some way in the operational sense in terms of what you can measure. And the surveying, then, involves the distance scale, therefore it involves this term. So if you want to measure the second-order time change, you must also measure the first-order distance change and the precise way in which you measure that will affect the experiment. For example, you could set up meter sticks, standard rods, which would involve only this term or you could survey the path by sending light signals up and down using radar technique. It would involve both this and this, since it involves the null geodesic. But whatever is done that must be taken into account. I think if one could solve the problem of time stability, then the other would become a detail. Let me say one more thing about the orbit precession; I remarked in the discussion yesterday that the effect of the distortion of the earth from spherical shape causes orbit precessions which are very large compared to the general relativity effect. Let me first write down the formulas. (writes equation on board) This is the general relativity effect that one would like to measure. However, if the earth is not spherical, as in fact it is not, (let's suppose it's a homogeneous oblate spheroid to make the calculation simple). Then it turns out that there is a precession due to the bulginess. This is not a general relativity effect. There is a bulge effect which is in the same units as $6\pi/5$ times a bulginess parameter times the ratio of the radius of the earth to the radius of the orbit squared. This epsilon is the difference between the polar and equatorial radii divided by one of the radii. This is a positive precession if the earth is oblate, as it actually is, and negative if prolate. Epsilon for the earth is about one part in 3 hundred, and if one takes a satellite at moderate altitude, that is, the radius not much bigger than the radius of the earth and the same order of magnitude it turns out that this factor is about a million times the relativity effect. It would be very difficult to pick it up superposed on this one. These results are for an equatorial orbit. I haven't done it for other orbits, but imagine that it would be difficult in general when you take into account other distortions of the earth besides the second harmonic distortion. Well these are some of the problems that one has in dealing with the information you can get out of the usual metric using the geodesic equations, that is thinking of mass points or of light rays. There are two different points which one can look at. The first of these is the possibility of picking up other components of metric tensor and particularly those due to rotation, called the Lenze-Thirring components. This has to do with the rotation of the earth; these are off diagonal components of the metric tensor that cross over between space and time components. They are not pure space or pure time the way they are with the static components. The second thing is to go beyond the geodesic equation (writes equation) and try to find things which don't depend just on the test object being a light weight or a mass point and the gyroscope experiment which is being proposed seriously now. We actually have request for reservation space on a satellite into NASA for this. This experiment is meant to accomplish both of these things. Now this particular experiment as Prof. Robertson pointed out yesterday, involves only the α and γ; it does not involve the β terms; it does not test the nonlinearity of the field equations, which is unfortunate because there doesn't seem to be any way of getting the β terms except through orbit precession which in a terrestrial experiment is exceedingly difficult. It does however work on the α and γ terms and that in addition works on these two. Now the experiment, in principle, consists in taking a spherical gyroscope, that is, a perfect sphere, (everything is perfect of course) spinning about an axis and supported in such a way that it has no extraneous torques exerted on it, and then simply observing this gyroscope or observing the direction of its spin axis over a long period of time. Now in the Newtonian theory the spin axis will not change at all, so that this is an example of the situation where the nonrelativistic theory gives you no effect, so that anything you absorb if it's not extraneous due to unbalance or stray torques, is a relativistic effect. You don't have to separate out a small correction factor from a big extraneous Newtonian factor. This is true even if the earth has an exceedingly inhomogeneous gravitational field because if the gyroscope is spherical, spherically symmetrical we say (it doesn't have to be a solid sphere), then the Newtonian gravitational field, no matter how complex it may be will not exert a torque. This result follows just from simple symmetry arguments. Now there is a possible but exceedingly small torque which can be exerted from special relativity effects; that is, if you have a spinning sphere with an axis something like this, then the material here at the equator is moving faster than the material at the poles. So because of special relativity, this material has a higher mass density because of its kinetic energy of rotation than the material at the poles. And therefore this thing has a slight mass quadrupole term in the diverging field of the earth, that is, in the gradient of the earth's gravitational field. There will be a slight torque exerted on this mass quadrupole. This is an exceedingly small effect far beyond anything that we hope to measure and far smaller than the general relativity precession. Hence this can be ignored, and there is no significant precessional effect for a perfectly spherical spinning gyroscope except with general relativity. So in that sense it's rather a pure experiment.

QUESTION: Is the elastic distortion bulge negligible?

ANSWER: No, not necessarily. This has to be worried about and it depends, of course, on the spin rate. What I meant there is something completely different and which arises even though the body remains perfectly spherical while spinning. There is an increased mass density due to special relativity because of the kinetic energy of the motion.

QUESTION: Isn't it so that an elastic distortion has the same effect?

ANSWER: Yes, an elastic effect will produce an equatorial bulge and therefore a mass quadrupole. The spin axis is not along the field line. They are at right angles to the field lines. There will be torque and this must be taken into account. There is an important factor with the instrumental parts, for example, mass imbalances or frictional torques or supporting torques of this sort and it is that they produce precessions which are inversely proportional to the angular momentum. That is, if a certain torque is applied to an object or to a gyroscope, the precessional angular velocity will be proportional to the torque divided by the angular momentum, or to the angular velocity of rotation. So that by varying the angular velocity, one can vary the precessional rate. On the other hand, the general relativity precession is not dependent on the angular momentum of spin and therefore will have the same value regardless of the rate of spin. In fact, it could be true, as Prof. Robertson mentioned yesterday, the geodetic precessions in a sense the property of the coordinate system which is traveling with the moving object, and therefore does not depend on it's being in rotation at all. Well, one can then do the calculation and see what the rate of precession is. I will now write down the formulas which are pertinent. If the spin angular momentum is called S_0, and I put the zero on to emphasize the fact that this is the spin angular momentum as measured by a comoving observer (this is essential), then this ω/f_0 precesses about a vector of ω, which is the precessional angle velocity vector, and ω then has three parts to it. I'll just write these down. If there is a nongravitational supporting force and the mass of the gyro is m, there is a gravitational term where r is the vector from the center of the gravitating body to the center of the gyro and v is the velocity vector of the gyro m is the mass of the gravitating body, maybe earth, and the third term I is the moment of inertia of the gravitating body assumed to be homogeneous and spherically symmetric, and ω is the angular velocity of the rotation of the gravitating body. Now let me say a little about the three terms. This is the special relativity term which our colleague Prof. Thomas discovered about 33 or 34 years ago which is important in atomic spectra and of course it appears here too. If you had a force exerted on a rotating object it produces an acceleration f/m and this reduces the precession of the spin axis. This is strictly special relativity. There is a very similar term, however, from general relativity. And this is the geodetic precession which was first discovered by deSitter in 1916 and later by Walker and it appears again in the literature of Pirani and I am sure others. I have not seen it in this form, I have seen it only in the form of an integrated secular change in direction over a cycle. This is the rotation term, the Lenze-Thirring effect which involves the angular velocity vector of the earth and the moment inertia of the earth. Thus we see change of sign, depending on whether r had an equatorial position where r is perpendicular to ω in which case this term is 0 and this is negative; or from the polar position where r is parallel or antiparallel to ω, in which case these two terms are the same sign but this is bigger than this, so that there's cancellation. I might just point out that there is an interesting relationship with the atomic case. In the atomic case you remember that the Larmor precession is twice the Thomas precession and of the opposite sign. Here if this force is just enough to hold the gyroscope against gravity it turns out that this term is three times that of the Thomas term and of the same sign. I don't know any reason for that. These are the terms then that one would like to measure and one can then consider how to do the experiment. But we consider doing it in the laboratory which means simply setting up the gyroscope under controlled conditions where you can see it and letting the rotation of the earth carry it around once every 24 hours. If you do that you must apply rather a large force here to hold it against gravity, and to keep it from falling to the floor. And then this term becomes of the same order of magnitude as this, it's about 1/3, and this term is of the same order of magnitude so that this is essentially an angular velocity of rotation in orbit which now is just the period of the angular velocity of the earth because the orbit is simply carried around with the earth, so these terms are the same order of magnitude and one has then three comparable terms. The other possibility is to do it in a satellite in which case it's in free fall at zero, g (or practically zero) the only effort required now is to restrain against the drag of the atmosphere, which at these altitudes is very very small, about perhaps 10^{-8} of earth acceleration. Hence this term is practically zero. This term now becomes the dominant one because the angular velocity of the satellite in orbit is some 15 times or so the angular velocity of the rotation of the earth, so this becomes the dominant term and this becomes quite a bit smaller. In fact, because of the 2/5 which appears in here and the 3/2 which appears here, and the ratio of the angular velocities, it turns out that this term is about 1/60 of the other term for a satellite at moderate altitudes, say 200 miles. This is in a way a stimulus to the experimenter. He can first do a "crude" experiment which measures this term and then he has the impetus to do a one percent experiment on that to get this term. I'm quoting Prof. Fairbanks. Now the relative merits of the laboratory experiment and the satellite experiment are the following. The one and only advantage of the laboratory experiment is that everything is immediately in front of you and under your control and you can see what you are doing. In the satellite, as you just heard so dramatically from Mr. Mitchell, things are tough and the controls may not be so good, the frustrations mount, and so on. That is certainly the advantage of the laboratory experiment. But everything else is against the laboratory experiment. In the first place, the fact that the gyroscope must be supported against earth gravity means that the support problem is very difficult. Because of very, very slight imbalance of the gyroscope, displacement of the center of mass from the center of support will be critical and the displacements are of the order of an angstrom or so in order to defeat this effect. By going to a satellite one reduces this term by a factor of 10^{-8} and has a much more favorable situation. The other advantage of the satellite experiment compared to the laboratory experiment is the fact that this term is increased in magnitude by a factor which is the ratio of the angular velocity in the orbit to the angular velocity of the earth. This term remains about the same and this now becomes much bigger so that one gains about 15 or so in the magnitude of the quantity to be observed. So there are these two factors which are in favor of the satellite experiment and one against. For these reasons we are now thinking in terms of the satellite experiment. In the satellite experiment then this term is approximately zero; this term turns out to be 7 seconds of arc per year, which is small.

QUESTION: What is it in terms of gr/c^2 - what numerical factor?

ANSWER: In terms of this quantity? (Yes)(It's some numerical factor times g) I can't quote it off hand but you can work it out: it's 3/2 of gm, if you take the radius to be the radius of the earth, it will be close to that, it's $gm/r \times c^2 \times \omega$ because r cross v over r^2 is $\omega - \omega$ for the satellite, that is, so this term is now gr/c^2, which is 3/2, and if you now let this accumulate for a year to time t, you have a factor of $\omega s \times t$. Hence it's the number of radians per year of orbit; 2π times the number of revolutions per year which is $2\pi \times 365 \times \sim 15$ circuits a day and then this factor 10^{-9} for the 3/2. That gives you the 7 seconds per year. First of all, one can vary these two terms with respect to each other by choosing the plane of the orbit. If one chooses an equatorial orbit then r is perpendicular to the earth ω. This term drops out and this term is simply $-\omega_e$ and this is $+\omega_s$, satellite angular velocity vector, so these two vectors have the same direction, opposite sign but the same direction, I should have given you the order of magnitude, this is about 1/4 second per year. By choosing a polar orbit, an orbit that passes over the poles, then we must average this over a revolution because r is sometimes perpendicular and sometimes parallel to ω_e. There is a cosine squared of the angle between r and ω that comes in here. In the case of the polar orbit, the precession due to the Lenze-Thirring effect is half as big as for the equatorial orbit, but it's now in a different direction from the orbital effect for geodetic precession. For example if you have the earth this way (draws on board) here's the axis of the earth and the polar orbit looks something like this then the geodetic effect would be a precession about an axis perpendicular to the orbit plane, but the Lenze-Thirring effect would be about an axis in the orbit plane so these will be at right angles. Hence, in this case, the small effect is not simply superposed on the big effect, adding or subtracting algebraically, but it is now in a different direction and may therefore be easier to find. I should call your attention to one effect which is very obvious though no one has mentioned it in this conference: that is the effect of aberration of the light. This effect will appear in any optical experiment which is done from a satellite. If you are looking at a star from any moving platform, there is an apparent shift in the direction of the star just due to ordinary aberration, which is equal to v/c. Well as you know the earth, in it's orbit around the sun, it's +20 seconds at one part of the orbit and -20 seconds at another part. This will be true also of a satellite going around the earth. The satellite speed is smaller than the earth orbital speed so that the periodic changes in aberration due to the motion of the satellite around the earth will be, say, of the order of a few seconds (this depends on the orientation of the star, of course, with respect to the satellite) whereas the 6 months variation due to the motion of the earth and the satellite around the sun will be a maximum of 20 seconds. So if one is comparing a telescope which is pointed at a star with the direction of the gyro axis, then one has to expect a periodic difference between these two because the gyro axis is not affected by aberration. The thing with which you are comparing, i.e., the apparent position of the star, is affected by aberration. Well this is a simple correction one has to take into account. It is well understood but it has to be remembered and planned in the experiment. Now I won't attempt to say anything about the experimental setup. If there are questions Prof. Little will try to answer them. I will ask Prof. Little to take over.

PROF. LITTLE: In Fairbank's absence I've been asked to say what he has been doing about this and give you some description of the experimental side. It's going to be more difficult than I thought because there seem to be quite a number of gyro experts here. I'll show what it is thought that can be measured. You can see why we are fairly optimistic about it. The size of the effect is rather small if you have an earth-bound laboratory. The precession is of the order of 0.4 second of arc per year but if you do it in a satellite it's somewhat bigger, about 17 times bigger, about 7 seconds per year. If you put these into reasonably usable units this comes out to be 2×10^{-7} seconds of arc per second. The present gyro is not quite as good as this and the uncertainty of angle, and the precession and jitter from this position is something, which apparantly if you have a good one, lies between 10^{-3} and 10^{-4} seconds per second. So one is asking for an improvement of the accuracy of something between 10^4 and 10^6 if you want to measure this a few percent. Now you can write down the defects of gyros and see what you can do about them so that you can measure this. I'll write down all of what is wrong with gyros: First of all the trouble is that they have bearings and these bearings are not perfect. There is a certain drag on the motion, Δf, I'll call it, a random force acting on the bearing, which is due to the mass of the gyro times the effective value of the acceleration, it is the support that the bearing has to bear which is something of that order. The second cause - suppose we have a gyro here, if the center of mass is not at the center of support but some distance, Δr away, these are separated slightly. This gives rise to a torque which is proportional to Δr times the mass of the gyro, times the acceleration of gravity. You must try to make these as close together as possible. Then there are some nasty effects which come about just due to the properties of materials. If you have a very large speed for the gyro, then the gyro, supposing it is spherical will begin to bulge (due to the centrifugal force). Then if the thing is not perfectly polycrystalline, there might also be a change of shape, which might give you a change in Δr, so that you'll get a $\Delta\dot{r}$, you might say. One such change is due to plastic flow. Metal or any other material under sustained stress will begin to give after a while and it will change it's shape and it will change the value of Δr. Then in addition to this, due to thermal expansion, there will be changes of size which might affect this and also there might be a change of shape. These things will affect the performance of the gyro if you let them occur. Fourth: If you have a metal gyro and if there are any magnetic fields about at all, the interaction of the magnetic fields with this gyro will cause random forces to act depending upon how the H field fluctuates. This field will give rise to a certain amount of torque as well, which will impair the performance of the gyro. Hence we should try to cut down on the torques due to eddy currents if you have a metal gyro. Then fifth: If you have this gyro running and it's not in a perfect vacuum then the gas molecules striking it, will give rise to random forces on the sphere. These gas torques will perturb the performance and the time precession of the gyro. Taking these things into account, one would think that the way to make a gyro to do this experiment would be to make one which should be run in a satellite. Now there are several reasons for this: The first one of course, is that the effect is very much bigger - the effect is something of the order of 18 times greater than on earth. So it really should be done in a satellite. The other point, from an experimental point of view, which is much more important to get the gyro to work at all, is that the g^* value, the effective value of the acceleration in the satellite is much smaller than on earth. In fact at 400 kilometers the deceleration of the satellite due to air drag is of the order of 10^{-8} g, where g is the value at the earth's surface. This improves things a great deal and everything becomes a lot nicer where the g^* value is as low as that. Yesterday there was some talk about compensating for g^*, if you could put gas jets on the satellite so as to reduce this still further then it might make the experiment even simpler than it is now. Then secondly, I think we must do this experiment not at high ambient temperatures but at temperatures of the order of 4° absolute. There are good reasons for doing this at low temperatures, the first thing, is that at these temperatures a number of metals become superconductors and this has several important effects. In a superconducter there are two things which occur, first of all the electrical conductivity becomes infinite and secondly, the magnetic induction within the superconductor with a certain correction goes to zero. If you take say a sphere of a superconductor and put it in a magnetic field H, then the lines of force avoid this sphere. You can make use of this in several ways. You can now make an extremely good bearing system. That is, if you take a sphere and you have a current loop down here you get flux lines something like this. If you put the sphere here, when it sits on the flux lines they will act as a cushion. Then we have a method of floating a sphere on a current-carrying loop so that you can form a bearing which has no surfaces which touch and which should have rather small residual torques. However, the torques are not absolutely zero. The field necessary to support this is proportional to H^2. If H is the field, then you require H^2 to give you the lifting force on this. So the value of the field H is proportional to the square root of g^*. Now in using a superconductor you do introduce some additional torques. This is, because, when you go into the superconducting state, the superconductors are not as perfect as we would like them to be. When you go into the superconductor, this B is not zero everywhere. There are certain trapped inclusions of flux in the superconductor and you get a certain amount of flux which is trapped here. Now this can interact with the magnetic field around about it, and can act on the sphere and give it a small torque, so that you then get a torque which is proportional to the flux which is trapped times the value of the field. Hence it is important to have the field H as low as possible or to get g^* as small as possible. But there is another useful feature about the amount of flux that is trapped. Just recently, Prof. Fairbank showed conclusively that the amount of flux which is trapped in the superconductor is given in units, predicted by London about 10 years ago, of hc/e; that is, the amount of flux that can be trapped occurs in integral values of hc/e. He did an experiment and demonstrated that this is almost correct, but with one difference, the units are $hc/2e$, the difference being that in superconductivity it is the electron pairs which carry the current. This is an important result because this tells you that if you have the initial field low enough, then the amount of flux you can trap is identically zero. Now there have been some measurements by Mendellsohn on a sphere and the amount of flux trapped appears to be zero up to a certain value of the applied field. Provided you start with a field which is low enough, it looks as if it is possible to reduce the trapped flux to zero. The next thing is that in a superconductor if you take a superconductor of this size with the vacuum outside here, and if you apply a magnetic field when it reaches the superconductor it doesn't drop to zero immediately but it penetrates in a small distance, and the distance it penetrates in is of the order of 500 angstroms. In this region, you find the number of electrons that can interact with the magnetic field and cause any kind of losses is not <u>all</u> of the electrons as it is in the normal state but only a small fraction of these electrons. In fact the number of electrons which actually interact with the magnetic field is given approximately by $\exp - 3.5T_c/T$, where T_c is the critical temperature of the superconductor, which is about 7° and T is the actual temperature at which you operate. This amount becomes exceedingly small if you got appreciably below the critical temperature. It means if you have a superconductor rotor, you have eddy currents which are scaled down by the effect of, first of all, the ratio of the penetration depth to the penetration depths of the metal in the normal state, which is a factor of something over a thousand, multiplied by this exponential factor which can be over a hundred or thereabouts, depending on the temperature at which you operate it. The second property of the superconductor is that you can cut down on the amount of flux which you might generate in different parts within the satellite by using the superconductor as a shield. If you put the superconductor about here, this is a cylinder, the total flux which goes through here must be a constant, just because $d\varphi/dt = 0$ so that you can shield the thing perfectly from extraneous magnetic fields by the use of a superconductor. Then to get rid of the difficulty of gas torques by operating this at very low temperatures the vapor pressure of all materials tends to zero as the temperature tends to zero. Thus if this was operated at a temperature of 4°, any residual gas which may exist at 400 kms could be frozen out by having a suitable baffling system, and you should get vacuums then which are much better. The pressure would be much less than 10^{-9} of a millimeter. You could get rid of residual gas torques that you had beforehand. Then as the temperature goes down, as temperatures tend to zero, you also find that the coefficient of thermal expansion, which is proportional to the specific heat of the lattice and this at low temperatures varies as T^3 so that at the very low temperatures the coefficient of thermal expansion goes to zero very rapidly. By operating somewhere down here at 4°, one can get rid of any thermal expansion and the difficulties which arise due to the change of shape or the change of size of the rotor. And secondly, one other difficulty lies in the plastic flow which occurs in the metal. If you take a temperature low enough, the plastic flow tends to zero with the absolute temperature so that you get excellent dimensional stability if you operate at very low temperatures. Taking all these effects into account Dr. Fairbank has calculated what one might expect would be the error in such a gyro and it appears that if g^* is held to 10^{-8} times the value on the earth's surface that the error in the gyro would be approximately 2×10^{-8} seconds per second, which would allow a measurement of the gyro precession (which Prof. Schiff has calculated) to a precision of the order of 1 percent, and the measurement of the Lenze-Thirring effect to something of the order of 30 or 40 percent.

Now there are some details which might be mentioned about how one can hope to do some of these temperature tricks at these altitudes. Refrigeration is particularly simple in this case. In order to provide refrigeration for a period of a year, one can take a hundred pounds of solid hydrogen and allow it to sublime. This will maintain temperatures of something of the order of 4° to 5° absolute. (It depends on the exact way in which you convect this without restricting the sublimation.) And by putting in a container of 5 liters of He^3 or He^4 you can maintain a temperature of something between 0.5 for He^3, and approximately 1° if you use He^4. It's necessary probably to spin the containers to keep the liquids in, or have a porous plug from which the liquids can diffuse or evaporate.

QUESTION: For what period?

ANSWER: For one year.

QUESTION: How big a structure?

ANSWER: Enough to carry about a hundred pounds. The density is about 1/10 per gram per cc and that would be 200,000 cc. You could keep your losses down so that the chamber on the outside could be a meter in diameter. The refrigerated compartment, that's what you want? It could be as big as a cubic meter.

QUESTION: You meant the helium to be in addition to the solid hydrogen if you want the lower temperature?

ANSWER: Yes, plus this if you require the lower temperature. The insulation is provided on this by glass and aluminum called super insulation. The rotor itself, what it could be made of is not certain. There are several materials. It could be niobium which is a suitable superconductor, it could be Vanadium, these two are extremely strong so you can use very high rotational speeds. It could be quartz or sapphire. The rotor itself would be a perfect sphere. There is an important point here, for determining the orientation without having to put marks on the sphere would be to measure the orientation of the sphere would be to measure the orientation of the sphere to something of an order of 10th of a second of arc. The way one does this is to take a sphere on which there is a small source and then have a Mössbauer absorber and a detector out here with suitable circuitry (draws on board). This is the cylinder and this part is the absorber. You determine optically the orientation of that flat, then the sphere itself rotates about some axis and this cylinder also has superconductor bearings that rotates about another axis here. Now the source in which the γ ray which comes through here and these both rotating together, that if the axes differ, so that one is like this then the source gets modulated as it goes around. From a position close together here as it goes around it gets further apart (draws sphere). If the axis were here, then at one instant they would be close together and at this instant they would be very far apart so that you get a modulation on this. And you would find that if these two axes were not exactly coincident you would not get resonant absorption. Mr. Bol has been working on such a detection system and has successfully obtained a result (last night, I believe). It must be shown here that the angle of variation changes with the counting rate quite markedly. This is done in a crude manner but one can detect here about a quarter of a degree of variation using very low velocities. The detection system is something of the order of 1/10 of a second of arc. I think the real facts are contained in the head of Dr. Cannon and Mr. Bol. This satellite should lie below the Van Allen belt because the heating due to the bombardment of the low temperature parts would be excessive if it passes through the higher radiation region. For Mössbauer detection it would certainly be better to keep away from this belt.

RECESS:

CHAIRMAN: I'll call for discussion on the two papers we have just heard on the gyroscope experiment.

AUDIENCE: In the case of Draper's gyroscopes, are the numbers that compare with these representative of these gyroscopes?

ANSWER: This is the value that off-the-shelf gyros are said to have. 10^{-3} is the oft quoted number. But 10^{-4} seems to be a perfectly achievable laboratory number.

CHAIRMAN: Would this be an appropriate time for you to make some remarks?

PROF. NORDSIECK: I'll take about 15 minutes to talk about another gyro which looks as if it might be a reasonable thing to put in a satellite for this experiment. A lot of the thinking is very similar to what you have heard just before. The only difference is that we at the University of Illinois support the gyro by electric Maxwell stresses rather than by making it superconducting and putting it in an appropriate magnetic field configuration. Another difference is that we have constructed gyros of this sort and have operated them and know a great deal about what they will do. I'll say just a little about that, but I want to make this a kind of commentary on the last talk. In the first place I am quite certain that the state of the gyro art is such that we can make gyros with drift rates good enough to do this experiment. Drift rates good enough in the environment in which they will be operating. I'm convinced of that. (It's nearly a freefall environment.) My approach would be different from the cyrogenic approach but I think in either of the two ways one can achieve low enough drift rates. But I think it's very questionable whether one can achieve high enough resolution of angular measurements in the satellite environment to do the job. Because I think we can make a gyro which in the satellite will drift less than the relativity effect. But I consider it very tough to measure the angles - measure them against stars, which is probably what you would have to measure them against, with sufficient accuracy. It is a mean job on earth to get a fraction of a second of arc and it's a meaner job still in a satellite. That's where I think the main problem will come. Not getting the adequately low drift rates but adequately precise angular measurements. That's just my general feeling about the situation. I thought when I came here that this experiment was impossible. Now I think it's almost possible and the reason is the 3π that I didn't know about. That's a factor of 10, and that makes a difference. I would agree almost with everything about the discussion that was given before. I got the impression when it's all said and done that the cyrogenic system is a very complicated system to put into a satellite and that worries me a great deal. I feel right now that the electric support would make a simpler system to put in a satellite because there is no cyrogenic complication, and we have analyzed the stray torques on this electrically supported object thoroughly and verified them in the laboratory. I'll put down a few numbers about that. Experimentally we now do less than one third times 10^{-8} radians per second at 1g in the laboratory. We know for sure how to get a factor of 30 on this in the laboratory so we can do 10^{-10}, and this is in the laboratory. Then as we pointed out, at least one of the sources of stray torque is proportional to the amount of force you have to apply to an object to keep it from falling on the floor. If it's in free fall, that component of the stray torque at least can be regarded as approximately proportional to the g field in which you are operating and there you would gain a factor of 10^{6} or so in going into the satellite envirnoment even without compensating for drag. Let me just list the causes of torque in this electrically supported thing. This is essentially the same list we had before: Gas drag: In this case there are electric torques - those are from the supporting fields; then there are magnetic eddy current torques, and those are essentially all. The motion of the axis due to mass unbalance I would regard as an item that you could tolerate because it produces a predicable motion of the axis. It's the unpredictable part of these torques that you have to worry about. Concerning this one the previous speaker made a great deal of it, saying that you have to get down to better than 10^{-9} mm of mercury effectively to make the gas drag tolerable. I don't believe that, because we have experimental measurements of the size of this effect in vacua that we create in the laboratory between 10^{-6} and 10^{-8} mm, and the gas drag can just be forgotten about if you do a proper job with the vacuum. It is negligible even for the relativity experiment at 10^{-8} mm. Whether you get the 10^{-8} mm by pumping or by taking advantage of the thinness of the atmosphere where you are, I don't know, that's a detail - you might not have to pump. We also know exactly how big this next one is and how it varies with the magnetic field which is, of course, with the square of the field, and this tells us how much we have to shield in order to get the torque due to that down to a tolerable value. And that again is a reasonable problem. The reason is that the torque goes as the square of the fields. So that if you put in a factor of a hundred worth of shielding you get a factor of 10,000 worth of reduction in torque. We know this experimentally. We put a single Mumetal can around the thing and got so much reduction in the magnetic torque, and then a double can, we know it works. What I'm trying to say is that of these three, and this is the whole list, this is the one we have to work on. The others are more or less understood and tolerable so that we can forget about them. This one is definitely proportional to the g field because if you don't have to support it at all, you don't have to put any fields on it so that on this one, one would expect to get some of this factor of 10^{-6}. Now I'm a pessimist so I wouldn't say that we could get all of the factor of 10^{-6}. But it seems quite clear to me that you could get something like a factor of 10^{3} or possibly 10^{4}, by putting a gadget in a more nearly freefall environment. Then this effect will go to 10^{-14} at 10^{-6} g. I'm not putting in a whole factor of 10^{-6} here because I worry. These things start to be more important. Relativity drift is about 10^{-12} radians per second. I really thing it's about this simple. All three of these are thoroughly understood in the sense that we can account for the performance of a laboratory instrument which is doing this well. The main reason for the factor of 30 here that we see but have not yet got is that the rotor that we use now starts round - it's made round when it's not turning and bulges when it turns - and this torque is all due to the bulge, the centrifugal bulge. You see if the spherical surface were ideally round while rotating you couldn't put a torque on it if you tried with an electric field because there is a very wonderful rule that says that an electric field must enter a conducting surface normally. There are a few tiny sources of torque here that I haven't mentioned, such as currents in the metal rotor due to the electric field due to the induced charges moving relative to the metal. Those are extremely tiny effects. I think there is one other that I've forgotten which is many orders of magnitude below these three. We have a very successful readout, which, however, depends on the rotor having a chosen axis of rotation, and that in turn means that the rotor is not spherically symmetrical in its mass distribution. It has appreciably more mass near the equator than a spherically symmetrical thing would. The logic of it goes this way, and for ordinary gyro applications, this is a very good logic. It may not be so good for this one because it introduces a complication with the gradient of the gravitational field. But the logic for ordinary applications of gyros is like this. No one has yet devised a good readout system. It may be that the Mössbauer effect is a good one in this sense. What I meant was an all-aspect one, one that doesn't require that there be a pattern on the rotor which is related to a selected axis. If you select an axis by making the thing heavier around the waist, then you can put a pattern on the rotor, which is related to that axis. You also have inherent dynamical stability about that axis, because things tend to want to rotate about the axis of greatest inertia. That's the way we do it; we make it slightly heavier about the equator. We put a pattern on it and observe the pattern optically when the pattern has about 100 complete cycles around the equator and the rotor turns at several hundred revolutions per second so that you get an enormous amount of averaging or filtering, and we are, with no difficulty, able to measure 10^{-6} radians with that existing readout scheme. However it's one that depends on there being a selected axis. Now the selection of the axis can be made by one percent difference in moments of inertia. That's enough. One percent, Leonard tells me, still gives you too much rotation of the axis due to the inhomogeneity of the earth's field. But this is an instantaneous effect, and the average effect over the whole orbit can be zero or much smaller if the orientation is correctly chosen. That's about all I want to say.

QUESTION: What was the figure you quoted, 10^{-8} radians?

ANSWER: 10^{-6} radians is easy, I would be quite certain that one could get 10^{-7} radians. This is the sensitivity of the readout system, and that gets down into fractions of a second. Now that's not the whole problem. What are you measuring? You are measuring the angle between the rotor axis and a framework. Fine, so you've got that to 10^{-6} radians. What do you do then? I don't think telescopes are going to do that well, not telescopes that you can fly.

PROF. DICKE: One way to avoid that trouble, it seems to me, is to have two rotors spinning in opposite directions.

PROF. NORDSIECK: They go the same way. If they don't go the same way, then two experts have told me wrong, and I think I'm an expert on that too.

PROF. FOWLER: I didn't understand how you are going to get this factor of 30. Is that by changing the shape?

PROF. NORDSIECK: Preshaping it. The rotor looks like this now, and I'm a very poor draftsman, but I'll try: that's the interior and this is the exterior and this is the axis. It's fatter here around the equator and as of now you make the exterior round by lapping techniques, round to 5 microinches or so.

QUESTION: What is that, about a centimeter?

PROF. NORDSIECK: 2 inches diameter, and I suspect slightly larger would be in order for this sort of thing, perhaps 3, 5, 6 inches in diameter. You gain in angular momentum stored per unit mass. Now this thing is made round within a few microinches and then it's turned, put in, and spun up, and the deviation from sphericity at operating speeds is in the order of a hundred microinches, or maybe even a thousand microinches, depending on the speed. It goes up as the square of the speed, and it's that factor that gives us this much drift. We feel that we can preshape it by making it a little thinner here just the way you figure a lens instead of making a sphere. You can make it by optical lapping techniques, so that when it turns at a selected speed, it's round, and that plus some other things that I haven't mentioned will give a factor of 30. A factor of 30 is not all due to that but most of it is.

PROF. FOWLER: That you have not done yet?

PROF. NORDSIECK: No, but we are morally certain we can do it. Everything in this gadget is analyzable. It's a very simple flexible dynamical system.

PROF. FOWLER: What's the problem on temperature control?

PROF. NORDSIECK: These performance numbers were gotten by merely thermostating the room, no other temperature control except thermostating the room. If you don't thermostat the room, you open the window and it's winter and it's one temperature in the day time and another at night. Then it performs about 10 times worse than that. We have no special temperature control on it. Now I think the temperature of the important elements which are the rotor and the support coils varies by $\pm 2^{\circ}$ or 3° F.

PROF. FOWLER: What I mean is, are you going to let this thing take up the ambient temperatures, or are you going to try to keep it at the temperature which you tested it in the laboratory, because all of these distortions are certainly going to be functions of the temperature?

PROF. NORDSIECK: If one would design this to go into a satellite, the way we would go about it would be, first, to decide the convenient temperature to operate in the satellite and then run it in the lab at that temperature.

PROF. FOWLER: Then the whole point is whether your refrigeration or your heating is any simpler than Fairbank's was.

PROF. SCHIFF: I think there is a tendency for people who are not involved in cryogenic work to think that the techniques are difficult. This is a hard thing for me to realize. Apparently these things are not difficult at all, they are quite simple.

PROF. NORDSIECK: By the same token another speaker said this morning that temperature is controlled by passive means.

PROF. FOWLER: Well how much weight would you say would keep this thing at 3° from some predetermined value?

PROF. NORDSIECK: Well, you choose the value.

MR. MITCHELL: It will depend on the structure that you have around it. It's my impression from the studies that have been made that using super insulation that you are using here, purely radiative shields, and controlling α and L/D that you can control it fairly easily. So that you can run the thing over a range of temperatures of $\pm 10^{\circ}$ and have a calibration.

PROF. NORDSIECK: There's no first order effect of the temperature.

PROF. TAUB: I think that is the point he was trying to get at, if in the laboratory you have a variation of 20° then you get a factor of 10 in performance . . .

PROF. NORDSIECK: And another point, the way the temperature comes in, is in the cross term between the temperature and the force you have to apply. Obviously if you apply more force, you need more torque no matter what the temperature is.

PROF. BERGMANN: Will these experimental techniques be affected by the total shape of the vehicle or in the satellite in which you propose to run it. I don't want to get into the technology of shielding that we got into yesterday but suppose it should be decided to work out a shielding in which the core of your laboratory is in truly free fall. Then it would seem to me that some of the questions in one connection may be quite trivial, provided the technology has been worked out. I was thinking, in terms of the cryogenics, that if you are going to evaporate 100 lb of hydrogen you may have torques of all kinds from the recoil as the stuff oozes out of the pores. You would refrigerate your shield, but here's the problem: whether it's good or bad, will it take on an entirely different shape?

PROF. NORDSIECK: I would like to say a word or two about this situation now. With this idea of the shield that shields the interior object from everything but the gravitational field, one should take a second look and try to decide whether the right way to do this is with either the cryogenics, or the electric support or any other support. Perhaps you should turn around and isolate the rotor totally from the external world except for gravitation instead of what both of these two gyros are trying to do, namely, to isolate the angular degrees of freedom but not the translational degrees. Now the only remark I want to make is that someone should make a very serious study as to what extent an object could be isolated inside another, translation-wise, as well as rotation-wise because you may come up with some peculiar things that are hard to beat (some things to do with work functions or magnetic effects of one sort or another). I think that perhaps the right way to do this job is neither with cryogenics nor with superconducting support or electric support.

CHAIRMAN: I think I will have to keep this from getting into a design session again. I think that the statement just made by Prof. Nordsieck is an extremely important one and let's have that in the minutes. These problems should be looked at, but by those who are concerned with making proposals of this kind.

PROF. NORDSIECK: It is by and large easier to isolate only the angular degrees of freedom without attempting to isolate also the translational degrees of freedom but I haven't ever tried to isolate translational degrees of freedom so I don't know how hard it is to get the forces down to such a thing, but someone should look.

CHAIRMAN: Phipps, would you care to comment on these gyroscope problems?

DR. PHIPPS: I'm from the Naval Ordnance Test Station; if our organization can be of any assistance in the design or construction we would like to cooperate. I don't have any specific technical suggestions. I might while I've got the floor just mention one sort of facility - this is a general educational comment. We have a facility, and there are several other military installations around the country, that have the sort of facility which most laboratory scientists don't think about; namely a supersonic sled track on which speeds of a 1000 or 1500 meters per second are easily attainable. The length of our track is 4 miles and I think there is one that is 6 miles. Accelerations in excess of 100 g's are attainable with light loads of less than 50 lb and loads up to many hundreds of pounds can be given lesser accelerations. These are speeds that are difficult to obtain for durations of seconds in laboratory confines, of course and are in fact about an order of magnitude faster than you could easily obtain in a laboratory space by means of linkages and so on. I would like to just apprise everyone of the existence of this kind of facility; just knowing about it might suggest some sort of experiment that someone might feel would deal with this kind of thing.

PROF. CANNON: I would like to indicate other differences between the two kinds of gyros that weren't mentioned specifically and then have you make some comments on it. The first difference has to do with thermal gradient effect. As Dr. Little pointed out, the coefficients of thermal expansion all tend toward null at cryogenic temperatures. At room temperatures they may be large so that the thermal gradient across the gyro could effect a mass shift. I think maybe we are starting to see the magnitude in this analysis. If, at drift levels of this amount the effect tends to be large only when you have a big change in room temperatures as you indicated, then the question is whether at drift levels of this magnitude, there are quite small thermal gradients that would have effects in these areas other than the effect due to the supporting force. That is perhaps due to electrostatic effects, electric torques, etc. That was the first question I wanted to ask about. Another difference between the two gyros, really in the sensor techniques in this case, is that with the electrostatic gyro you sense the location of the sphere itself, the body coordinates by which you measure; whereas with the Mössbauer technique that Fairbank and Little are working on, and with other techniques if possible, you are measuring the location of the instantaneous spin axis. And since the effect that we are measuring is the drift of the momentum vector rather than the rotation of the body coordinate system, it's really the spin axis that we want to sense. To the degree that they are coincident in the electrostatic gyro you succeed. I wanted to ask the question; How are you able to start this exactly about the principal axis? Presumably if this is not exactly where you have your pattern you could make a correction for that then? Well that was the second question. The third question has to do with complication. The cryogenic gyro is passively stable. The field supports it in a stable position and, as it moves off, the field itself passively returns it to neutral. With the electrostatic gyro, a servo technique must be used. With regard to equipment and the design situation on our pinhole system Mr. Langley made some calculations last evening which showed that you could quite easily maintain the location of the sphere to one micron using only the gas which would normally be sublimed off from the hydrogen supply.

PROF. NORDSIECK: Could I answer? The one about the thermal temperature distribution in the rotor. What we have is a rotor which is a piece of metal of reasonable conductivity and very thoroughly isolated from the outside world so that the temperature gradients that exist in that are very, very tiny unless there is a nonuniform source of heat. And we understand all of this, you can have a nonuniform source of heat and get leakage currents across the gaps. We know all about this, and that again goes away as the supporting forces go down so that it would be just as good at constant temperature. What was your next question? (the next one is about this spin axis)

PROF. NORDSIECK: Oh, about instantaneous spin axis versus angular momentum. We measure the instantaneous spin axis also, and the instantaneous spin axis in general precesses about the angular momentum vector, but tends toward it. It happens after it has run for a few hours.

QUESTION: Is it damped in a few hours?

PROF. NORDSIECK: Yes, and whether it's damped or not, you measure the average condition of the angular momentum. It goes around once per revolution almost and you measure the average of the spin axis. It's many minutes with respect to the body but it's not many minutes with respect to the observer.

PROF. LITTLE: If you run these rotors for a sufficient length of time, you can see the effect of slipping which will undoubtedly occur when you run these at high speeds.

PROF. NORDSIECK: We have seen no effect. We have a very sensitive measure because with the system used, the readable voltage with a gap which is very tiny, less than 10 thousandths of an inch, you can measure that to 1 percent or 0.1 percent. If the rotor swells up or shrinks down you can detect it very readily with a voltmeter. As a matter of fact that's how we know that we can isolate it. We know that when you first start it involves so much energy that it takes a half a day to cool off and we can tell that the voltage keeps drifting upward, so we have a very sensitive means of telling whether it's swelling up. There is a designer's choice about how fast you run this thing.

PROF. BERGMANN: I would like to raise a problematic question. It seems to me that at the present time we are discussing details of experiments that we are not even sure ought to be done or not. What I mean by that is the following: Every one of these experiments appears to call for quite major resource at this stage of the technology though they might be trivial 10 years from now. It seems to me that a conference in the nature of things should not decide if these experiments should be done.

CHAIRMAN: There is no attempt to do this, but I think it is well to air the important issues. Expose them to the air and put them aside and let them simmer.

PROF. BERGMANN: I think so, but I think one of the things that can be done is to present a number of possibilities that may suggest that certain types of systems of technology would be desirable because they are flexible, would be useful for a larger variety of experiments, but I think that this is the kind of information that can come out of a conference because a lot of people present different ideas. You cannot try at this stage to make a decision between magnetic versus electric gyros.

CHAIRMAN: And we are not trying to.

DR. ROMAN: I had a nontechnical comment that I'd like to make. Dr. Schiff mentioned at the beginning that he had approached NASA for a reservation of space for this experiment. I would like to say that we would like to be informed as soon as anybody begins thinking seriously about an experiment which is going into a satellite or a probe and we would like to be kept informed as progress goes ahead as to how it's going and when it looks like it's going to be ready. But as far as a firm reservation of space, this is not done until something between one and two years of lauch and it's not done until we have a reasonably good idea of how long it's going to take to get the experiment ready, that there are no really major unsolved problems such as the sort of thing we have been discussing here today. Perhaps the primary reason for this is that once you have a firm assignment of space you are on this nasty schedule that Jesse Mitchell talked about and you've got to produce this week, that week, and the next week down the line without any major hitches and this obviously can't be done at this stage of an experiment.

CHAIRMAN: Any comment?

QUESTION: I wonder if Prof. Schiff could remark on the suggestion of full spin axis. It appears from the equations written on the board that the angular velocity has a singularity independent of the spin axis. Is that correct for the precession?

PROF. SCHIFF: You mean the precessional angular velocity? Yes, the precessional angular velocity vector is independent of the spin axis.

QUESTION: So the opposed spins then would have their axes changed if they started parallel.

PROF. SCHIFF: No, just the opposite. Here is the precession axis. Any vector, no matter what it is, will precess around this thing. It's like planets precess around the sun. Nuclear magnetic resonance is all it is. I thought people might be interested in one concept we had which was that the gyro rather than the telescope would be the thing that would control the vehicle. The telescope would be used simply to monitor how far the stars had moved with respect to the vehicle. And I also wanted to ask whether there is any basic reason why the electrostatic gyro and the cryogenic could not act as a team, I mean both kinds of gyros.

PROF. NORDSIECK: I think some of the advantages might be that the stability might be better and nothing could be worse. I still think the telescope will be the biggest part of the payload.

PROF. SCHIFF: Could I ask someone from NASA, perhaps Dr. Roman, what the status of the telescope situation is now? That is, how good a telescope do you intend to put out and how well will it be focused, etc.?

DR. ROMAN: Well I don't see any good reason why we won't be able to put up a 36 inch telescope. This is still several years off. Perhaps with the present schedule it will be toward the end of 1965. As far as how soon we will be able to control the vehicle, Jesse gave you those figures this morning. We are aiming for a minute of arc without the telescope signal and the accuracy with which you can control it with the telescope depends on two things: First the accuracy of the control system which we are designing for a 10th of a second of arc. Like most design figures we don't know whether we are going to make them but we have hopes and secondly, this should not be ignored in this problem, the diffraction limitations of your optical system which are comparable with your 10th of second of arc for a 40 inch telescope.

AUDIENCE: A comment on that is, in this case, it is not an easy reading that we need but we may average it over as long a time as we need to take.

PROF. DICKE: We have a little device down in a hole in the ground with an aperture this big. I think I computed a signal noise basic to the light intensity as 10^{-8} radians which is about one or two seconds.

DR. ROMAN: I might mention one other problem that comes in here in answer to this question of accuracy. When I say a 10th of a second of arc, this is relative to a position. In other words, you get a position, and you hold to that position for an hour or two. This does not mean 10th of a second of arc relative to some other part of the vehicle which may change by appreciably more than that.

PROF. DICKE: What I was going to say is that I think the problems are rather systematic errors here rather than the diffraction limitation. The diffraction limitation is not a problem of your source brightness. You have enough protons coming in but is is a problem if you can't build the kind of instrumentation that is going to be free of systematic errors, and if you are operating that's another thing.

CHAIRMAN: Well I'd hoped this morning to have Prof. Weber's paper but it's not feasible. The proposal is as follows that we have lunch and reconvene at 1:00; that we have a short coffee break at 2:30; and then afterwards we have a formal kind of discussion in which the group acts as a panel of the whole.

LUNCH:

CHAIRMAN: Prof. Weber, "Detection and Measurement of Gravitational Waves."

PROF. WEBER: One of the most central issues in relativity theory has always been the question of the existence of gravitational waves. Thus far no one has observed such waves. Until last year no exact solutions of Einsteins' field equations were known which might represent spherical gravitational waves. A number of theoretical issues have been resolved in recent years. At the moment I think it's safe to say that many physicists believe there are gravitational waves. If we consider the problem of the observation of these waves for a moment, two kinds of experiments suggest themselves. First, experiments modeled after the classical experiments of Hertz; that is, one would like to be able to generate such waves and detect them within the confines of a small laboratory with relatively modest equipment. The second kind of experiment concerns the possibility of detection of such waves if they are being generated someplace outside of the earth; that is, the possibility of the detecting of interstellar gravitational radiation. Thus we would like a detector which might be responsive to interstellar gravitational waves, if there are any. For both of these problems, the measurement of dynamical gravitational fields in paramount. So our first task therefore is to proceed on the assumption that perhaps there are gravitational waves and to talk about apparatus for the measurement of dynamical gravitational fields. We have to recognize the fact that you will have to use gravitational and nongravitational forces for such apparatus. Suppose we consider first a mass point which is moving along a world line and let's imagine it's subject to both gravitational and nongravitational forces. We start with this action function $I = -mc\int ds + W$ and this will be part of the action function representing the nongravitational forces. If we carry through the variational principle in the usual way, we find that the equations of motion for this mass point look like this: x is the coordinate and this $\Gamma^{\mu}_{\alpha\beta}$ is the christoffel symbol of the second kind; F^{μ} is the nongravitational force, m is the rest mass. So this equation looks very much like the geodesic equation but the right-hand side involves the nongravitational force. Now this equation reminds me of the $F = ma$ which was written this morning. This is nothing more than a generalized way of saying that $F = ma$, but we are describing things in such a way that F is the nongravitational force. If we talk in terms of the four-velocity u^{μ} which is dx^{μ}/ds we can write this first equation in this form. Now here we have the covariant derivative of the four-velocity with respect to s and this is F^{μ} the nongravitational force divided by mc^2. But now we have to recognize another fact; that is, if we wish to detect the presence of waves by some sort of local measurements, we can only do this if we have a laboratory apparatus and if we absorb the relative motion of one part of this apparatus relative to some other part of this apparatus. So we must consider at least two mass points, or perhaps an assemblage of mass points. So supposing we do this, then we will have not one world line but a series of world lines and we might label each of these world lines with some parameter say V_1, V_2, V_3. And these lines would be lines of constant x, so that now, remembering this v parameter, if we differentiate this covariantly with respect to v in this fashion, and then if we introduce a differential vector which is tangent to the lines of constant v, corresponding as far as the v lines are concerned to the four-velocity which is tangent to these s lines, then if we work with this equation and change the order of differentiation, we can write it in a form in terms of this infinitesmal four-vector N^{μ} this way. Here we have the second covariant derivative of the N^{μ} with respect to s and here we have the Riemann tensor coming in, and the four-velocity again and the four vector $N\beta$. And on the right-hand side we have an object which tells us how the nongravitational forces change as we move, say, from one part of our apparatus to another part of it. The simplest application of this equation of motion would be for something like two masses connected by the spring. The spring then furnishes the nongravitational forces. We rewrite this equation in a special coordinate system. We imagine that the time axis runs in the direction of the tangent to the world line at the center of mass; then we pick a geodesic coordinate system and write the equation in this form. (Perhaps we should do one thing before rewriting this in a special coordinate system, we should note that m is a vector which connects one mass point to the other.) If we want to talk about displacements in a covariant way we should write the $m_{\mu} = r_{\mu} + c_{\mu}$ and this r_{μ} is supposed to be a sort of constant vector and the displacements will be determined by this c_{μ}. So if we say that the covariant derivative r_{μ} with respect to s is always zero more or less and insert this in here, and go over to this special coordinate system we have with the case of the two masses connected by a spring, the second derivative of C_{μ} with respect to time plus we imagine we have a dissipation force plus a restoring force tensor in this fashion (writes equations on board). So what we end up with then for the equations of motion of the displacement vector for these two masses is the equation of motion for an ordinary harmonic oscillator. The driving force is the Riemann tensor. Thus if we observe things like relative displacements, or more precisely, strains, then the observation of these strains gives us a means of determining certain components of the Riemann tensor. Now it turns out that one can deduce these same equations of motion in a number of different ways. One could deduce them, not from an action principle, but from the left side of Einstein's field equations or from the right side making use of the appropriate form of the stress tensor so there is probably not a great deal of question about equations of this sort. Now the immediate thing which arises is about the sensitivity of a detector of this sort. If one talks in terms of the objects which are used by physicists, one likes to think about things like the absorption cross section for such an antenna. Well what is the maximum absorption cross section for such an antenna. Now this really depends on the manner in which the antenna is damped. We know that we have a familiar result in classical electromagnetic theory. If we have a radio antenna the maximum absorption cross section is of the order of a wave length squared. This comes about because the radio antenna is radiation damped, and the radiation damping for an antenna of moderate size is usually the dominant effect. One can transfer energy from this antenna to some sort of apparatus in such a way that the absorbed energy is equal to the energy which is scattered in consequence of the radiation resistance of the antenna. The same arguments could go through for the gravitational wave antenna. One might say what is its cross section if it is radiation damped? Well, if it is radiation-damped, the absorption cross section turns out to be roughly a wave length squared corresponding to the electromagnetic case, which is no surprise. But actually the absence of the constant of gravitation from this formula for the cross section causes some rejoicing which turns out to be very premature. If one actually calculates what the radiation damping is, one finds that in the gravitational case it's incredibly small. For an antenna large enough to fit on a table the radiation damping corresponds to the antenna executing something like 10^{34} cycles before its amplitude drops by a factor e. So that other irreversible processes within the antenna are really orders and orders greater than the radiation damping. And this is a very fundamental way in which the gravitational wave antenna differs from, say, the electromagnetic wave antenna. A fact that the internal dissipation is always orders greater than the radiation damping is taken into account. One can calculate that the absorption cross section for such an antenna is 15π times the gravitational constant times the mass of the antenna, times a quality factor q and a $4\pi^2$. Let me just rewrite this in a somewhat different form, as the quadrupole moment of the antenna times the quality factor times $4\pi^2$ over a gravitational wave length squared divided by $8\times\omega$ times the speed of light. So one sees then, practically speaking, if the antenna is not damped by radiation resistance, the constant of gravitation does change; the cross section unlike the radiation damped case does depend very much on the kind of antenna one has. It's directly proportional to the quadrupole motion of the antenna. This quality factor again is the number of cycles of oscillation for the free antenna for which its amplitude will decay by a factor e; c is the speed of light and ω is the angular frequency. I just wrote this in a slightly different fashion than I have it here. Really it's a little more meaningful if I write it this way. If I don't show the quadrupole moment explicitly, I'll have a βr^2 so that this object is simply $2\pi r/\lambda^2$. If you write it this way it shows you just how the ratio of the linear dimensions to the actual wave length enter into the cross section. This formula itself really turns out to be not so awfully meaningful because in gravitation theory there are issues concerning energy flux and energy localization which we do not have in electromagnetic theory. So if one talks in terms of the measurement of the Riemann tensor or the power spectrum of the Riemann tensor, one is talking about a very meaningful thing. If you are talking about cross sections, well the specification of the energy flux in terms of the Riemann tensor is something which is almost a matter of choice. So by choosing different frames of reference, by choosing different forms for the gravitational stress energy pseudo tensor, one could obtain different values for the cross section. These values are obtained by simply making the most pessimistic assumption; that is, assuming that the observer is in the rest frame of the generator and taking the canonical stress energy pseudo tensor. I think that this apparatus should be regarded more as a device for measuring the power spectrum of the Riemann tensor than as something that has a well-defined cross section in terms of energy flux. This has nothing to do with apparatus, it just has something to do with the status of general relativity. So much for this. From this formula it looks as though by making the spacing of the two masses arbitrarily large, one can get as big a cross section as one wants. A more detailed analysis shows that this is not really the case because the restoring forces for an object of this sort are transmitted with the velocity of sound and not the velocity of light. So when the spacing approaches the wave length of sound in, say, the spring, one finds that one actually gets the maximum cross section there. So this object βr, in practice, is likely to be something like 10^{-10}. Then the cross sections turn out to be about 10 orders smaller than you would think at first glance. Well, to understand how the velocity of sound enters into problems of this sort one has to consider the interaction of an elastic body with gravitational waves. To extend these equations of motion to an elastic body isn't so difficult. One could say that one is talking about the interaction of say gravitons and photons if one wants to talk in those terms, or, speaking classically, an interaction of the normal modes of say an elastic solid with gravitational waves. Carrying out this sort of ananlysis, one finds that he can write an equation of motion for the strain tensor in an elastic body which looks something like this. This is not a strain tensor, and now I'm talking for the moment about an isotropic body. We have a dissipative part and an elastic part (writes equation). I guess this is as far as we need to go for the moment, so that one has here an equation which is similar to the wave equation of acoustics. We have a wave equation for the strain tensor in a solid; if we transpose this to the right-hand side this equation then tells us that the normal modes for say an elastic solid can be driven by the Riemann tensor. Observation of the normal modes of a solid, then, gives us a means of detecting or observing the Riemann tensor. Now here again, in order to solve a practical problem one can introduce a geodesic coordinate system, at, say, the center of mass of an elastic body. This is a pretty good approximation because the velocity of sound is about 5 orders smaller than the velocity of light so that in terms of the gravitational wave length one could have an extended region of a solid; that is, the region of a solid many acoustic wave lengths across which would still be small in terms of the wave length of the gravitational wave which excites it. Also, the fact that the acoustic waves might be small in comparison with the gravitational waves means that one could do a fair job of sampling the gravitational field. That is, one needs really a rather small object rather than a rather large object. If one considers the solution to this equation, a number of consequences appear: First, the solutions say that if you have a fixed mass and just vary the form factor, the cross section goes through a maximum when the length is the order of one acoustic wave length, justifying the earlier result. Also if one applies this equation to the normal modes of the earth itself, he finds that at higher frequencies, an apparatus located on the surface of the earth will behave as though it were in free fall. This is the consequence of the fact that the acoustic waves travel much more slowly than the gravitational waves. One might say that the platform on which the apparatus rests isn't conscious of the acoustic excitation of the earth because by the time the acoustic wave from the center of the earth has arrived at the apparatus, then something else has actually happened. In terms of the attenuation of the acoustic waves it is possible to show that if one considers the motion of the surface of the earth to be due to the gravitational forces plus a component due to acoustic waves, the acoustic wave component is exceedingly small under the circumstances, that the frequencies used in the apparatus are large in comparison with the fundamental mode frequencies of the earth itself. Perhaps instead of talking about the cross section, one should talk about effective quadrupole moment. If one has an extended elastic body whose normal modes are interacted with gravitational waves, the equivalent quadrupole moment is of the order of magnitude of the mass of the body times the acoustic wave length squared. The fact that the cross section goes up this way with mass suggests that one is going to need a rather large apparatus to set good limits on gravitational waves. The apparatus that we are constructing right now has a vacuum chamber 10 feet long and 6 feet in diameter. The acoustic resonator has a mass of 1-1/2 tons and is suspended in such a way as to insulate it to a large degree from the acoustic distrubances around it. In addition to this kind of apparatus which is really determined by space, weight, and cost, the presence of this mass term suggests naturally that one might use the normal modes of the earth itself as a detector. So after calculating this we were delighted that the Cal-Tech Seismology Group were able to identify the earth's normal modes in the Chilean earthquake and they were able to observe the mode noise for a considerable period after the Chilean earthquake. Unfortunately the earth's normal modes have quite a bit of noise associated with them. This noise level is mainly a consequence of the winds blowing over the earth's surface. So that all of the earth's modes are excited to some degree by this wind system. Now some of the earth's modes have a quadrupole character and would be expected to couple to gravitational waves and others don't have the right kind of symmetry. Of course it would be extremely nice if one could show that certain modes could be excited and other modes were not excited. The wind noise precludes this and the only thing we were able to do was to set some limits on the gravitational flux, limits on the Riemann tensor from the Cal-Tech seismology data. This was published in Nature, on February 11, 1961. These data are useful only for setting an approximate upper limit, and as I said, the 54-minute mode has a q factor of 400; that is, it undergoes 400 oscillations before it's damped by a factor e. It showed a mean square strain of about 10^{-23} so we interpret this to mean that the power spectrum of the $(R_{iojo})^2$ components of the Riemann tensor could not have exceeded 10^{-75} per centimeter fourth radians per second. If one makes the most pessimistic assumption for the energy flux, associated with this power spectrum for the Riemann tensor, we would say that if the power spectrum were more than 20 watts per square centimeter per radian per second, it would produce a bigger effect. However, if one had made a different choice for the frame of reference if one had assumed that the center of the mass of the earth is say in free fall relative to a supposed radiator then energy flux limits are 10 orders smaller and of course that much more attractive sounding could be attained. However, only the data on the mean squared Riemann tensor, 10^{-75}, is meaningful here. The fact that there is this very high noise level associated with the earth's strains, and the fact that this is connected with the wind system, suggests the use of the normal modes of the moon as a detector for gravitational waves. It turns out that the cross section of the moon's normal modes for gravitational waves is something like hundreds of square meters, I calculated it but I don't remember what the figure was. This is, of course, small in comparison with the optical cross section of the moon but it's really not an incredibly small thing to talk about measuring. One might wonder what the background noise associated with the moon's normal modes would be if there weren't any winds. Well there would certainly be some other sources of noise. I can think of a few and I started to calculate a number; one can't be sure that the ones one might think of and calculate will be the ones he will actually find. So we are awaiting with some eagerness the results of this work which is under way to study the seismic effects on the moon. Now if one considers the sun here and the earth and the moon as the moon goes around the sun, there are stresses and strains on the structure of the moon and these are not at all harmonic as the moon rotates so the harmonics of the moon's period should then be present for driving forces for the normal modes of the moon. What I am in the process of doing now is calculating this effect and assuming that it is the only source of noise, which is extremely doubtful, and trying to find out just how good a detector for gravitational waves the moon would really be. In addition to the excitation of the normal modes of an elastic body, the Riemann tensor of an incident gravitational wave can also induce rotations. If one calculates this, one ends up with a formula which is superficially rather different from the one Prof. Schiff wrote down this morning. But on closer examination, they reduce to the same thing for this kind of drive. If we consider the influence on the Riemann tensor on the rotation of a system of masses, an extension of the earlier argument shows that one can write an equation of this sort. The sum over all the masses, and this object is the Levi-Civita tensor density, the left side is a kind of rate of change of angular momentum. This would be a body in free fall with no nongravitational forces (writes equation). This would be a rigid body with no nongravitational restoring forces and here one can use this equation to ask questions like, what would be the effect of incident gravitational waves on the rotation of the earth? It turns out that for the rotation of the earth, one can use this equation to derive a formula containing the mean squared fluctuation and the angular momentum, divided by the square of the angular momentum. Again one makes suitable assumptions concerning the stress energy pseudo tensor. This turns out to be about $25\pi g/\omega^2 c^3\ t_{or}$; t_{or} is the flux of the incident gravitational radiation, ω is the period of rotation. If we apply this to the earth and if we assume that all the known anomalies in the earth's rotation are associated with gravitational waves, we arrive at a flux, a total flux in this case of the order of 10^8 ergs per square centimeter per second; again this might be modified by a factor of 10 if one made a different assumption concerning the motion of the center of mass of the earth relative to some assumed radiator. This figure of 10^8 perhaps sounds a little better if we talk in terms of watts, 10 watts per square centimeter - a big number, but the size of the number indicates what a small interaction gravitational waves have with matter. Now to go back to some of the things that we said earlier. We said that the spacing between the two masses of a gravitational wave detector or the extension of, say, an elastic body had to be of the order of an acoustic wave length. This is true provided the restoring forces are transmitted with the speed of sound; one can imagine arrangements such that the restoring forces are transmitted with the speed of light. This is a hard thing to do but is doable. I think that rather than a 10 order improvement over the kinds of apparatus with which we are working now, one might expect perhaps a 5 order improvement. Also the apparatus which we are building now makes use of the compressional modes, and for practical reasons one would do better if one used something like a torsion pendulum. The cross section of the apparatus we are building now is probably of the order of 10^{-17} squared centimeters, while for a torsion pendulum type of apparatus of about the same dimensions, one could get perhaps about 5 orders better than this. In addition to the use of the restoring forces transmitted with the speed of light, one can partially accomplish this if one makes use of the piezoelectric effect. The equations for the piezoelectric apparatus become quite complex so I won't write them down. I'll just indicate that a piezoelectric device is a nonisotropic solid, so one has to write the equations for an anisotropic medium with the coupling of the various kinds of stresses. We have only done this for the simplest case, the one-dimensional case, and it turns out that one does actually gain something over the simple acoustic type of resonator but nothing like a 10-order improvement. So the present situation then with regard to detectors is that one can build apparatus for observation and measurement of the Riemann tensor and such apparatus is much less sensitive than the corresponding apparatus for observation of the Maxwell tensor. Such apparatus need not necessarily respond to gravitational waves. This apparatus will respond to any Riemann tensor regardless of its origin just as a radio receiver or an atom will undergo a transition in the presence of fields. The atom doesn't care whether these field are a null radiation field or are applied by the experimentalist locally by means of a signal generator or things of this sort. If our apparatus does show something, there will still be the issue of whether it is the Riemann tensor really coming from interstellar space or whether we are observing some phenomenon in the interior of the earth itself, which gives us a Riemann tensor of 48 components within the frequency range of our apparatus. In addition to these things, the apparatus is of course useful for studying the dynamical near-fields associated with objects within the laboratory for the first time. These dynamical near-fields are of interest in themselves. I should say that the possibility of a Hertz type radiation field experiment is out of the question for the immediate future, but it's not beyond the bounds of possibility that certain other interesting components of the near fields, the ones corresponding to the Faraday law effect in electrodynamics, might hopefully be seen if our present effort is multiplied perhaps by an order of magnitude. At this stage, I might echo what Pound said yesterday, if it's multiplied by an order of magnitude perhaps they had better get some other manager. So now let's discuss the problem of the generation of such waves. Historically the generation was discussed very early by Einstein and by Eddington, the problem they considered was the radiation of gravitational waves from a spinning rod and perhaps this result plus the detector cross-section problem has discouraged people for some years. The result which Einstein and Eddington got for a spinning rod was that the radiative power is 1.73×10^{-59} I_m^2 ω^6 ergs per second. I_m is the moment of inertia of the rod. This formula is in some sense misleading. The implication here is that you just keep increasing the rotation frequency ω and get as much radiation as you want. But you could only do this up to a certain point. You have a spinning rod. The rod is going to ultimately break in consequence of stresses. There is an ultimate angular velocity for any given rod, and it turns out if you talk about a given length for a given rotation frequency, the only length you can really construct out of the density and the elastic modulus of the rod is the wave length of sound. It turns out that within an order of magnitude or so, if you rotate the rod at an angular frequency ω, its length cannot be larger than about a 30th of the wave length of sound for that ω; otherwise the rod will break. The implication of this is very bad. If you have a rod of a given length that rotates, the wave length of the gravitational wave will always be at least 10 million times the length of the rod; hence if you rotate a rod which is one meter long in the laboratory, the waves are 10 million meters long. It's rather difficult to think of a wave zone experiment under those circumstances; also, if one recasts this formula in terms of the maximum allowable stresses, one finds that the larger the rod, the more radiation one gets from it. So that large or slow moving rods are better than very short high-speed objects. The work of Beams and others has shown that one can rotate small objects with angular velocities of the order of a million radians per second; but such objects are extremely small and the radiation one gets from such objects is also small. The radiation damping time for a one-meter rod is something like 10^{30} years. One can do better than this if one considers the weak-field solutions of Einstein's field equations (writes equations). If we take the field equations and consider the weak-field approximations so that the metric tensor is the Lorentz metric plus a first-order part, and if we define a new object this way, h being the trace of this object, and if in addition, we impose the coordinate conditions, then we find that for this object, the weak-field version becomes proportional to the d'Alembertian of $\varphi_{\mu\nu}$ so here we have an equation which is very similar to the set of equations in electrodynamics with the important aspect that the source for the gravitational field is the stress energy rather than the four-current. If one thinks in terms of an oscillating volume-integrated stress tensor, this oscillating volume integrated stress tensor will have the same effect as some volume integrated current in electrodynamics, except for a rather large numerical factor. If one then talks in terms of an extended object, say a crystal, and sets up a system of stresses in this crystal, if the crystal is going to break as a consequence of these stresses, the rupture will take place over a plane instead of over a point as in the case of the spinning rod. Also, if one has a crystal which is many acoustic wave lengths long, then the stress tensor produces two kinds of effects. For acoustics, well, for an elastic body, the stress tensor is something like this. This is pressure plus energy and the 4 velocity, this object is pressure, so the stress tensor is linear in the acoustic pressure and under ordinary circumstances this acoustic pressure is the biggest term in it. Thus if one has a crystal which is many acoustic wave lengths on a side, this acoustic pressure term would be expected to lead to rather larger effects than for a spinning rod. For a crystal one acoustic wave length on a side, one gets an effective quadrupole moment, which is the mass of the crystal times the wave length of sound times the displacement amplitude of the ends divided by a factor 2π. For a bigger effect where you have an extended object then one runs into trouble because since the stress tensor is linear in this acoustic pressure, alternate sections will be oscillating out of phase with each other so that an extended crystal will be very much like a large assemblage of quadrupoles each one oscillating out of phase with its nearest neighbors. So if you want a big effect, you don't get it from this term directly. All this term gives you for an extended object is something of the order of magnitude of a single resonator, one acoustic wave length long. However, this term which is quadratic in the pressures comes into play so that if one has a crystal which is the order of magnitude a gravitational wave length on a side, then this is about 10,000 acoustic wave lengths. Then one finds that this term is in fact larger than this one and one begins to get effects which are rather larger by many orders than the effects for a spinning rod at the same frequencies. There are other ways of getting around the difficulty that neighboring elements oscillate out of phase with each other. If one studies in detail the piezoelectric effect, one can find that in consequence of the polarization charges, there are ways of driving a piezoelectric crystal off resonance so that one gets a rather large volume integrated stress. The effective volume is something like gravitational wave length cubed. How much better can you do than these other methods? It turns out that if you specify the frequency, the use of a large crystal in this way gives one the 40th order of improvement over the spinning rod. By this step, one has a rather vast improvement. Unfortunately, in terms of numbers it would have been better if one had been able to achieve a 50th order improvement because 50 orders is just about what you need to do a modest laboratory-type experiment. If we talk about apparatus which is perhaps a meter on a side, we are still about 10 orders away from being able to do a Hertz type of wave zone experiment. The problem can be formulated in a different way, if you formulate the problem by saying, suppose it were a matter of national pride like getting a satellite out, how big an apparatus would you really need? How much money would you need? Well, how much money one would need is something I don't think I could estimate. How big an apparatus you would need is perhaps some kind of figure of merit for our present technology. I get that you could do this with crystals perhaps 100 meters on a side. Let's be safe and say crystals of the order of 100 meters on the side. Well I think this means one shouldn't do it - at least in the immediate future one shouldn't do it. Although I must admit that a walk through the laboratory next door, yesterday afternoon, made me think twice about this, also when one thinks of a 2-mile long accelerator, one just wonders how ambitious one ought to be. So I might sum all this up by saying that a Hertz type of experiment is very likely out of the question in the immediate future unless one multiplies the effort by a really extraordinary amount. And even if one were to multiply the effort by this extraordinary amount there is of course no guarantee that one would be successful in a reasonable time. What can one really do? Well one can construct apparatus, we are doing this, to measure the Riemann tensor and one can go about and measure it for the dynamical local fields which we can produce in the laboratory. With this apparatus we can explore these local fields, and hopefully learn something post Newtonian about them. Just how much we can learn we don't quite know. We are at the moment immersed in a lot of practical problems. Problems which are by no means impossible but which do take quite a bit of time to solve. I won't mention them; they are known to everyone. In addition to this I think that one ought to watch the progress of satellite technology, particularly as it pertains to the moon. I think it may turn out that the moon is really an excellent detector for gravitational waves, if these calculations and the seismic experimental data indicate that the moon is quiet, then I believe we ought to think in terms of a rocket landed, moon-crust, strain-measuring apparatus to give one some information on the normal mode strains of the moon itself.

CHAIRMAN: Discussion?

PROF. SCHIFF: I didn't understand, he mentioned a factor 10^{-10} in the case of the earth as a detector, having something to do with the motion of the center of the mass of the earth.

PROF. WEBER: Yes, let me put it this way. Supposing there were a source of gravitational waves on the moon, and supposing we set up our apparatus at the center of the mass of the earth. If the center of mass is in free fall, then it's certainly appropriate to use it in a geodesic coordinate system. The Christoffel symbols vanish; all the standard forms for the stress-energy psuedo tensor vanish, and you reach the conclusion that your apparatus is infinitely sensitive in terms of energy flux. Now of course, this is nonsense. If you go one step further and say that the apparatus isn't really at the center of the earth, it's on the earth's surface, then the appropriate objects to insert in the stress-energy pseudo tensor are the first derivatives of the Christoffel symbols times the radius of the earth. This gets one something like an acoustic wave length over the gravitational wave length quantity squared. This is where the factor 10^{10} comes from. This corresponds to the fact that, if you like, by coordinate transformation you can always transform the gravitational flux away. If you choose to transform it all away, you can tell everyone your apparatus is infinitely sensitive.

PROF. DICKE: There is one problem there, is the thing you measure coordinate independent?

PROF. WEBER: The thing you measure, well, I don't know that it's coordinate independent. If you take a geodesic coordinate system, then the thing that you measure is this object, certain components of this object.

PROF. DICKE: You have a dial and I don't think that dial knows what the coordinate system is.

PROF. WEBER: Well I think it actually does because - let me outline how I'm going to do this experiment. Are you ready. The first thing I do is throw this chalk to you (you should have caught it), then the first thing I determine is that this room in not in free fall so I correct for that. If the room is in free fall I could set up a geodesic coordinate system in a simple way. I know I could always do this, if I could do it in a specially simple way.

PROF. DICKE: But you'll end up reading something independent of the coordinates you choose and for that reason when you make a measurement it's going to be quite independent of any questions of a coordinate system.

PROF. DE WITT: Well, you are essentially measuring Riemann tensor here. Through the weak-field approximation the Riemann tensor is coordinate invariant.

PROF. BERGMANN: I think the following statement would be, in slightly less objectionable language, to measure the relationship between the Riemann tensor and the local velocity vector associated with the earth's measuring system.

PROF. DICKE: The measurement system interposes some tensor properties combined with the field properties of the system; It's an invariant that you construct this way which you measure.

PROF. WEBER: Let me outline the measurement procedure, the way it might be carried out. Supposing this is a piezoelectric crystal. Here is a voltmeter. Let's say this is a root mean squared vacuum tube voltmeter with some sort of filter so that it measures the root mean squared voltage within the response band of the apparatis? This voltage one measures, and one imagines that one has a certain coordinate system and the orientation of this relative to the laboratory coordinate system allows one to infer that the readings of this voltmeter correspond to certain values of R_{1010} in this laboratory frame. Now if you wish to rotate the crystal and then read the voltmeter, perhaps we get R_{2020}, and so on. So it's a matter of interpreting the readings of a voltmeter in terms of the components of the Riemann tensor calculated in this laboratory frame.

PROF. DICKE: But now in the same sense that you have chosen a simplified coordinate system, a particular coordinate system which is convenient, you are defining unique energy transport with this apparatus.

PROF. DE WITT: You can do that only in terms of the Fourier components of the Riemann tensor itself. You can't very easily get a local flux density.

PROF. DICKE: If he chooses a proper coordinate system, proper in that his apparatus will be fixed in this coordinate system . . .

AUDIENCE: Well he was just pointing out that if he did that in different ways he got quite varied values of the canonical stresses.

PROF. DICKE: He still is going to measure something which one ought to be able to describe in terms of local instruments. I mean he could heat this thing up and measure with a thermometer.

PROF. WEBER: You can do this in the electromagnetic case, but you can't do it in the gravitational case. If one happens to pick a coordinate system such that -- if this is the reference point for my apparatus, and if I just happen to pick a coordinate system such that all the Christoffel symbols associated with the source are zero, and I can certainly do this, then all components of my energy tensor are zero.

PROF. DICKE: At that point.

PROF. WEBER: At that point, yes. Now I can of course average them over the apparatus and if I do this, I'll get a number which will be perhaps 9 or 10 orders different from the number I would get if I had chosen my coordinate system such that the Christoffel symbols didn't match.

PROF. BERGMANN: This is clearly all conceptual nonsense.

PROF. DE WITT: You can't use this canonical stress tensor, that's all it says; You can't transform the Riemann tensor away.

PROF. WEBER: That's why I'm not arguing too vociferously about this cross section.

PROF. BERGMANN: It doesn't make a hoot of difference whether he picks a geodesic coordinate system or any other coordinate system.

PROF. DICKE: But what he measures is an invariant.

PROF. WEBER: Of course, what I measure is an invariant. If I want to interpret that invariant in terms of an energy flux, then there is no unique way to do it. Is that statement correct?

AUDIENCE: Well I agree that this measurement is invariant with respect to the crystal, but I think with various approximations one can introduce a notion of the integral of the energy for various systems that one is talking about and essentially do this relative to the Minkowski frame or Galilean coordinate system. The results you get will be independent of coordinates. I don't know how one computes the cross section, all I know is that one can talk about the flux of energy and take into account what would happen.

PROF. WEBER: Total energy -- the problem of the energy localization is not a solved problem.

PROF. BERGMANN: There are two types of measurements in principle that one can attempt aside from the apparatus in which one can make a purely local determination of the gravitational radiation. The purpose of your equipment is more or less to do that. The other would be to relate what happens locally to a conjectured Minkowski-Lorentz frame at infinity. The latter would be a global investigation. The difficulty with a local determination in principle, is that obviously no radiation is detected because of the presence of a nonvanishing static gravitational field. I suspect though that it is not too conceivable that there are situations, maybe not on the earth, but somewhere in the universe where the radiation is orders of magnitude more intense than the static field, in which case it would be close enough to take whatever we measure as the measurement of gravitational radiation. But I think the danger that Dicke pointed out, namely that if you cannot formulate the experiment in terms of invariants, there is the danger that something is wrong because every clean experiment can be formulated in this way.

PROF. WEBER: I think in principle I can proceed to measure all the components of the Riemann tensor. I can calculate the curvature scaler and that's an invariant.

PROF. BERGMANN: The curvature is zero in vacuum so you'd better not measure that.

PROF. WEBER: You're right.

PROF. BERGMANN: Scalars are present in any nontrivial field whether they have radiation or not so you'd better not measure those either. It is quite difficult to say what should be the curvature with radiation.

PROF. WEBER: The apparatus certainly does measure the components of the Riemann tensor. And as far as I'm concerned that's the only real thing in the radiation problem, the Riemann tensor.

PROF. BERGMANN: No, no. You can have a nonvanishing Riemann tensor even in the absence of radiation. How are you going to tell the difference between a static field and static field plus radiation.

PROF. WEBER: The apparatus does that.

PROF. THOMAS: Suppose you have incoming waves and outgoing waves of the same energy, you'd use this then?

PROF. DICKE: No.

PROF. HECKMANN: If you insist on measuring the Riemann tensor, your main problem is that you are detecting in the first approximation only the classical field of Newton expressed in the language of Riemann. That's all you find in the first approximation.

PROF. DICKE: He is looking for a time dependent effect.

PROF. HECKMANN: That's another thing.

PROF. WEBER: This apparatus measures the Fourier transform of the Riemann tensor. All our apparatus measures is the time components, the Fourier transform of this object in the vicinity of $\omega = 10^4$. Now why $\omega = 10^4$. Well, as low an ω as possible is desired so if $\omega = 10^4$, a vacuum chamber 6 feet in diameter and 10 feet long is required. So to do $\omega = 10^3$, you would require a vacuum chamber a 100 feet long and I hope not 60 feet in diameter.

PROF. BERGMANN: Now suppose you have a static field, what I would call a static field, a Schwarzschild field, now you introduce on top of this Schwarzschild field a so-called coordinate wave, which clearly means a poor choice of coordinates and nothing else. You will then get an oscillatory component of the Riemann tensor simply on the grounds of your choice of a coordinate system. How are you going to tell this apart from a true gravitational wave?

PROF. WEBER: Well I think I can do this, if I can just do this experiment again of throwing this piece of chalk to you, then the first thing I would do is set up a coordinate system in my laboratory, then I would throw a piece of chalk to my friends and carry out certain other measurements and then I'd find a locally Lorentz frame. I might be a little more sophisticated. Now if I'm not really in a locally Lorentz frame I might have to put my apparatus in an elevator and twist it because I see the pendulums that wander around and things like that.

PROF. BERGMANN: But we are not concerned with that. We are concerned with coordinates plus the radiation frequency of 10^4 cycles that could deceive you into believing you had a gravitational wave.

PROF. WEBER: But if I choose such a crazy coordinate system, I'm going to discover that this is not a Lorentz frame, by throwing a piece of chalk and by carrying out all apparatus and showing that the special theory of relativity is valid within the confines of my room except for Riemann tensor effects.

PROF. DICKE: I think Joe introduced a coordinate system way back that would rule that out anyway. Didn't you say that your coordinates were going to be such that you were going to have geodesic coordinates plus the small variations. You wrote down the equations for the small variations.

PROF. BERGMANN: Not by any means, because this business of throwing chalk doesn't help you one bit.

PROF. WEBER: Isn't it possible to pick a locally Lorentz frame and to do experiments to discover that you have done this in a reasonable way so that special relativity is valid? What I do is set up my Lorentz frame here within the confines of this square, so anything I do in the confines of this square will be affected by the Riemann tensor. Now I bring in apparatus which is so big and now the effect of the Riemann tensor over this extended object begins to show itself but my coordinate system is something which I selected a long time ago. And I know perfectly well it's not one of those crazy oscillatory types.

PROF. BERGMANN: Well, as I pointed out before, your "crazy" has no standing in the body of mathematics. You have to say exactly what you mean by crazy.

PROF. DICKE: It has no invariant significance.

PROF. WEBER: I have no intuition for the carrying out of measurements in a coordinate system other than either a Lorentz frame or a Lorentz frame in which the spatial parts are spherical or cylindrical coordinates so that when I look at my laboratory and decide how I'm going to interpret my voltmeter readings, I pick a Lorentz frame and I can determine experimentally that I've done this in a sensible way.

PROF. BERGMANN: I think that perhaps the following statement can be made: In the literature people are thinking very actively about what is to considered to be a wave and what is not and the discussion isn't closed. I think the answer locally speaking is going to be something like the following, in the absence of the gravitational field, that is, in the absence of a gravitational wave but in the presence of a Schwarzschild solution, we have a Killing field, which defines among other things the time axis. In the presence of a gravitational wave the superimposed Killing on the static field gets lost. This is at least an invariant statement. Whether it has anything to do with the experiments I'm not sure.

PROF. WEBER: The only thing the apparatus measures is the Fourier transform of certain components of the Riemann tensor.

AUDIENCE: Well I follow what Bergmann says. Granting this then, one has enough to define what one means by radiational energy because one has to then set up conservation equations which will involve the gravitational part.

PROF. THOMAS: The difficulty is that you have static fields of various kinds, and you can superimpose an incoming wave and an outgoing wave and see if they compensate each other. Then you have to introduce somewhere the assumption that there is no incoming field at large distances if you are going to define what you mean. It's almost impossible to do this by a local experiment.

PROF. BERGMANN: Your standing wave field would destroy the Killing field. You could get no Poynting vector.

PROF. SHERWIN: Could I ask a question here? You mentioned looking at other near field effects. What sort of effects are you looking for? Are you going to move masses around? Velocity dependent effects, or something of this nature?

PROF. WEBER: The kinds of things we can do are the following. If we have a detector which is a 1/2 ton rod suspended in the vacuum chamber, then we can take a second rod in its own vacuum chamber and acoustically drive it in an alternating system of stresses. Then we can observe the interaction of the one with the other through the gravitational fields (they are both in vacuum chambers), and hopefully we'll have enough confidence in our experimental technique that we will be able to know that the interaction is through the gravitational field and not through acoustic leakage over the walls of the room.

PROF. DICKE: That's a direct interaction of the static field.

PROF. TAUB: Don't you have to go at least a wave zone away in order to see . . .

PROF. WEBER: Well, one has to go a wave zone away in order to see the radiation fields; I think one just needs to measure to extraordinary precision to see post-Newtonian fields in the near zone.

PROF. DICKE: I think you can look for a phase difference.

CHAIRMAN: Sherwin, are you satisfied with this radiational theory?

PROF. SHERWIN: I guess so. Is this going to vibrate the rod in a similar structure?

PROF. WEBER: Yes, we just drive it, so that the near-fields of this aren't simple.

PROF. SHERWIN: This is just the Newtonian field?

PROF. WEBER: Well, they correspond to the $1/r^2$ field of a mass point if you like. This isn't a mass point, it's an extended object and you are close to it. So the amplitude and phase of the fields have a very complicated dependence on distance from the edge.

CHAIRMAN: Prof. Bergmann has asked for some time to discuss a similar topic.

PROF. BERGMANN: I would like to talk about an experiment which could be done in gravitational waves and which are an order of magnitude away from Weber's experiments. But I would like to say the only reason I had the courage to ask for the time at all was that I think some of the experiments we have talked about are also one or several order of magnitude below present techniques. This I think is going to be more. I have in the discussion a minute ago mentioned that in order to do the experiment in gravitational waves, one might think, in principle, about experiments involving local properties, so that we never go out of the confines of our laboratory and global experiments in which we make explicit reference to the presumed existing Lorentz frames at infinity. Now what I want to talk about are some attempts at studying global properties of radiation. First, the question of radiation damping of the double star system. This is undoubtedly in the literature and in Pauli's Encyclopedia article. The rate per revolution at which the double star system will lose energy as a percentage of the originally present classical energy, a dimensionless quantity, should be given by the ratio of the Schwarzschild radius of the masses making up the system divided by the separation of the double star system raised to a certain power, which I will call n for the moment. This I guarantee because there is no other way we can form dimensionless quantities. The question is, what is the value of n. We figured out at lunch what you get, 3/2. If anybody here happens to know the right answer, I would appreciate it. But this rate of damping of the double star system, if you make other extreme assumptions regarding its composition this ratio might be as large as 10^{-5}. That would correspond to dwarfs of the mass of the sun having a separation of the order of the earth-moon system, so if you raise 10^{-5} to the 3/2 power you would get $10^{-7-1/2}$; less extreme assumptions will give a less favorable result. It might conceivably be possible to discover some such double-star systems, though I don't think there are any known at present. Now another property is I think from a conceptual point of view much more interesting; namely, that in the presence of gravitational waves, it is impossible at infinity to define in a unique fashion a Lorentz frame. That fact has been known under different guises, I think, for several years. There is a statement that leads in this direction in the paper by Bondi and co-workers that appeared a couple of years ago in the Proceedings of the Royal Society and I know that Penrose told us the same thing in a seminar. Here let me say what the effect is. I think it has an effect that is, in principle, observable but probably not observable in the near future. Supposing that you have a system which at one stage of its history emits gravitational waves and which has an outgoing light cone of a certain thickness. The question is, now, of course the amplitude of the waves, is going down by $1/r$, so you would think if you go out far enough there would be a possibility of an asymptotically Lorentz coordinate system, but this is not the case. Supposing that you go here to spatial infinity in the region of space time in which the wave has already passed and you set up here a tetrad of directions, one time-like unit vector and three space-like vectors. You ask, "Can I, far away from the source of radiation, all over space in a unique way define a parallel direction to this one." This is clearly a property of a Lorentz frame in flat space.

PROF. WEBER: Excuse me, are you talking about a Lorentz frame over the whole space or just one part?

PROF. BERGMANN: No, I'm talking of the Lorentz frame that is to exist everywhere in space isotropically except within a sphere of radius ρ or whatever you want, from the source of radiation.

PROF. WEBER: You were not implying here, when you said you couldn't introduce a Lorentz frame, that Fermi's theorem is incorrect, namely that you can always propagate a Lorentz frame along a given world line?

PROF. BERGMANN: No, what I'm saying is this that if you try to find an asymptotically integrable affine connection in the half space corresponding to spatial infinity after the passage of the wave, this is possible. This construction of the theory has nothing to do with Fermi's theorem. Secondly, if you do it at spatial infinity before the wave has passed you can also do that.

QUESTION: Either before or after?

PROF. BERGMANN: I'm sorry let me repeat my statements, since apparently I said it too fast. I say in the whole semi-infinite time, following the passage of the radiation at spatial infinity you can find an asymtotically integrable affine connection; that is the affine connection provided by the Christoffel symbols that is asymtotically integrable. I don't think anyone has ever doubted this. Then I repeat the statement that for the semi-infinite time prior to the passage of the gravitational wave that is, if you are here and by more than one devious route both avoid the proximity of the source of the physical system and the gravitational wave, and you go over here, the results are independent of the path by which you have displaced from here to here. I'm not using Fermi transfer, but ordinary parallel transfer. This is what I call asymtotically integrable because it's asymptotic in the sense that obviously the goodness of the thing depends on the minimum distance to which we approach this nasty region. If you get too close you get burned. You have to stay away from the disturbance. Now secondly I say, likewise if I confine my attention to the semi-infinite time preceding the passage of the gravitational radiation, the same statement may be made - that again I have an asymtotically integrable affine connection. Now neither of these statements is controversial or in fact startling, but what I want to say now is that if you try to hook up these two things to each other intergrability is lost. That means if you take this tetrad again and displace it parallel to itself by a path which at one point crosses the radiation cone, then continues here, and then appears once more in the radiation cone in the opposite direction and finally comes back, you will arrive here with a different direction from what you started out with and the degree of difference depends on the separation of these two short distances where you pierce the radiation cone and is independent of the distance to which you recede to infinity. That is you cannot save your skin by saying I'm staying 10 to the umpteenth light years away from the source. This will always happen. Now I can again make an estimate of the degree of discrepancy or uncertainty in the direction. I have done this for a double star system and again with the same conditions. This formula I'm willing to guarantee because I did this several weeks ago rather than today at lunch and it happens to come out in dimensionless units, that is, in radians, or in terms of v/c if you take the rotation of a time-like vector, that is, if the radiation is due to a double-star system.

CHAIRMAN: I'm surprised at that coefficient. I thought I had some relevant thing but apparently don't. Did you take a look at what the half life of the system would be?

PROF. DICKE: Two white dwarfs about earth's radius apart you get something of the order of a day almost.

CHAIRMAN: What I have is 10^{25} years.

PROF. DICKE: That was for normal double star though. This is for a double system of two white dwarfs.

PROF. BERGMANN: The period of rotation here is 3 seconds.

PROF. WEBER: I would just like to make one comment about the issue of the demand of the experimentalist measuring some invariant quantity. This is very very nice. But may I remind you when Hertz made his measurements he used the electromagnetic waves and the only invariants that I can describe for the waves that he used are both zero, and as far as I know every experiment which has ever been done using electromagnetic waves has made use of these invariants which are both zero. Since you have a null field both invariants match. Anything you ever measure in spectroscopy doesn't measure these objects. You measure things like the components of the field tensor which are not invariant objects.

PROF. DICKE: I think there is an important point to clarify here, we are not talking about the invariants of the field but we are talking about an invariant that you form by combining an apparatus quantity of some kind with a field quantity, and what Hertz measured was an invariant.

PROF. WEBER: In that sense what I measure is also an invariant.

PROF. DICKE: Oh, yes, it must be.

PROF. DE WITT: I think one is inclined to think in terms of cosmical implications of gravitational radiation. I wonder if Joe would care to comment on the following: If the universe were bathed in gravitational radiation what would be the indications on star motion, and how could gravitational radiation pressure contribute to an expansion of the universe? And also if the typical wave length of the gravitational radiation were large and if the solar system or galaxy would be shifted back and forth would this be detectable by looking at parallax of distant stars?

PROF. WEBER: Well perhaps I can answer the cosmological question by quoting from a remark which was made by Prof. Wheeler in the Solvay Congress of 1958. Wheeler said that the density of gravitational radiation could be as high as 10^{-29} to 10^{-28} grams per cubic centimeter, corresponding to a thousand ergs per square centimeter per second and still be consistent with present information about the rate of expansion of the universe. He notes that if this radiation were set free by the same process which causes the inhomogeneous collection of matter in the galaxies, it would be characterized at that time and also now by the same scale of lengths, of the order of 10^{24} cms, a 10^6 years vibrational period and this would correspond to a typical change in the metric of the order of 10^{-4} which is plenty big enough to measure by our techniques but unfortunately much too slow. The essence of this is that one might argue that on the basis of what is known about the universe, the gravitational radiation could be more than enough to detect. Unfortunately, it's a Fourier transform that's peaked in the vicinity of one cycle every million years, which is of course much too long to wait. On the issue of the astronomical effects I should recall the result that if one proceeds on the basis of not too naive assumptions, then a gravitational flux of the order of 10 watts per sq cm would produce anomalies, in the earth's rotation period of about one part in 10^{10}, say over a 3 month period. This is rather a colossal energy flux and one sees from this that the effect is extremely small so that it could surely have escaped detection.

RECESS: Friday Afternoon 2:30

CHAIRMAN: We are approaching the wind up session of this conference and I would like to state extremely briefly what my impression is concerning the matters of interest which have been raised here, and insofar as they might affect NASA. In the first place, it seems to be fairly obvious that the greatest promise and the greatest interest was expressed in the gyroscope experiment. The gyroscope experiment is a very delicate one and has as a kind of satellite on it the Lenze-Thirring effect, which is also of great interest in principle and which could not be completely dissociated from the gyroscope experiment. What would happen then in terms of the scheme I started the meeting with yesterday would be that we would have a measurement of this quantity $\alpha + 2\gamma/2$ which is clearly, in a sense, a linear effect. It involves only these two first-order terms in the coefficients of the metric and gives a determination really at this point of γ. Associated with that, would then be, the Lenze-Thirring effect which arises from the off-diagonal elements. But this effect is also in a sense of first-order because one would arrive from the first-order approximation to the field equations by having the Laplacian or the d'Alembertian of h_{oa} equal to the components of the stress tensor T_{oa}. So these things would be something that would involve this. T_{oa} **dxdt** is, of course, a second-order effect, except that the two velocities involved are, the velocity of motion of the field-producing body and the velocity of the one we are examining. Thus we have a quadratic velocity effect coming in here. It seems to me that the interest shown in this experiment, the problems which might arise, the discussions of eliminating drag are all relevant and indicate that this is a possibility which ought to be very seriously looked at. Schiff and his collaborators should be encouraged to go on with this in the hope that something could be done with it. There are a number of other satellite experiments which might contribute to the subject. Of course the question has been raised concerning the cosmological part. It seems to me that the general programs of astronomy which are not designed particularly for testing the relativity theory are all very much to the point and are of very great indirect interest at least for the relativity theory. I'd like to have Prof. Dicke discuss some more aspects of things that have been concerning him and things which some of us have called perhaps Mach effect which he has not. Let me say one thing about the Mach effect beforehand. In a sense, this is related to the Mach effect in that we get here an induced angular velocity which is of the order of the mass in length units, divided by the diameter of the object that we are comparing it with. And this kind of thing has long suggested a connection with the true Mach principle in the sense that if you integrate this thing and don't worry about the fact that the space you are integrating it in is now no longer a Newtonian one, if you integrate that, you do come here to a quantity which could be interpretable as the mass of the universe in these units divided by the radius of the universe - the same kind of result which Dicke has gotten in his theory. And I also had hoped to get Prof. Schiff in here for this day at least of the conference so that we could hear some more things relevant to this, in terms of the theory about which he has been talking. Bob, I'd like you to take on and say what you think about this.

PROF. DICKE: I would like to say two things, owing to the lack of time yesterday, there is one thing I think should be said, I'm sorry I didn't get it done yesterday, and I think it's sufficiently important that it should be said. I'm not going to take more than a minute or two to say it. You probably all remember this article by Cocconi and Salpetre that goes back 3 or 4 years, and this has been followed by other articles. I think there must have been 6 or 7, which discussed the possibility of saying something about Mach's principle in the following way: that if we think of the acceleration of matter in the universe as seen in a particular coordinate system as a source of inertial reaction, then one says that we are in a galaxy and this is a flat mass distribution. As a result of this one might well expect inertial reaction to have a tensor property, and that this tensor property, this tensor inertia, would show up in experiments in such a way that one would see a directional dependence of the inertial mass of matter. You would measure this simply by accelerating matter and seeing what force is required to accelerate it. I think this idea that Mach's principle would imply that such an anisotropy in inertial reaction should exist in a measurable way is a misunderstanding of Mach's principle, and I would like to say why that is. Now that's not to say that these experiments are not important because I think that the great accuracy obtained by the Hughes group on this and the great precision with which one says that one has as a space anisotropy is an extremely important result. I have only a quarrel with the interpretation of this. And let me say what the source of my worry is. First of all, one might crudely write some bad mathematics in this way: You would normally write equations of motion in the form of $dm.u^{\mu}/ds = F^{\mu}$. This constraint is not satisfied in general.

PROF. WEBER: But hasn't one already given up special relativity?

PROF. DICKE: Oh, no! You are still defining your four velocity in a way that this is an identity. Let me say that you have $ds^2 = g_{ij}\, dx^i dx^j$ if I divide this through by ds, I have $1 = u_i u^i$ with $u^i = dx^i/ds$.

PROF. WEBER: One doesn't have to do that. Suppose one insists on that equation of motion.

PROF. DICKE: Then I would like to know what you mean by u if you are going to insist on that equation, then you have to tell me what u is.

QUESTION: This is something other than the four velocity?

PROF. DICKE: I'm not really quarrelling with the possibility of writing the equation. All I'm quarrelling with is this specific equation. It is possible to write equations for which this condition is satisfied; this is not the important point, however. The important point is that the interpretation has been made by Sciama and by other people about what one means by inertial reaction. It means this, that the inertial reaction you get from Mach's principle is independent of the kind of matter you put there. Now to put it in these terms, if this is true, it doesn't matter whether I'm talking about an electron, or proton, or ion, or what. I'm going to have the same tensor properties. In other words the tensor properties, the inertial tensor, the tensor property of this is going to be independent of the kind of particle I put there. Now if that is true, I merely need to consider what the resulting tensor inertial properties are like at very high velocities, and it seems to me completely reasonable to assume that as I go closer and closer to the velocity of light globally, the resulting anisotropy and inertia of the proton should be that of the photon, and all fields should have the same inertial properties. Well, if you write this down consistently, as far as I can see, it simply means that you've written down equations for which the inertial tensor turns into a new metric tensor. The equations have the same form they had before just by redefining what you mean by the metric tensor. I haven't see any other way of doing it. If you are going to grant me that these inertial tensors are universal inertial tensors, it would apply to all matter. Then I don't see any way of getting an observable effect out of this; I think it always cancels. If this interpretation is correct, then I would say that the experiment is extremely valuable for another reason. It shows us with a very great precision that the inertial tensor-like property of a proton is the same for the electron or other fields. The universal character of this inertial tensor is a thing which experiment shows. It doesn't show that it has no inertial tensor property.

PROF. WEBER: I'm not sure that I understand your statement that a given mass having a tensor property corresponds to just a change in the metric.

PROF. DICKE: Well, let me show you how you can get from a variational principle tensor equations of this kind that do make sense and if you simply start in a known given metric field, assume that I have some general tensor m^i_j equal to a mass of the particle times some general f^i_j type tensor, which is symmetric. I'm going to assume that this is a symmetric tensor. Then I construct an invariant (writes equation). If I take this to be the variational principle, as everyone knows, I think, I will get equations that look like this (writes equation).

PROF. WEBER: Well, I don't think you need to go any further on it.

PROF. DICKE: Let me just write down the rest of it. Now this does have the property of an inertial tensor. It has the property that the constraint is satisfied automatically, the constraint condition. I'm not sure how much this can be generalized, whether there are other invariants that one can write that will serve or not. Thus you are rather strongly forced into this equation, and this equation is nothing but a new metric tensor.

PROF. WEBER: Sure, this embraces some other concepts like the invariants of ds for example.

PROF. DICKE: I don't know that this is a new concept; I'll just define this.

PROF. BERGMANN: This may not be the proper time . . .

PROF. DICKE: No, it isn't - not with the redefined metric tensor it is not; it's an arbitrary invariant.

PROF. BERGMANN: This variation principle is invariant with respect to the choice of coordinates. (puts equation on board) That principle is invariant with respect of the choice of coordinates it will come out the same way no matter what.

PROF. WEBER: I don't question that at all. I'm just questioning the interpretation of the ds in terms of the time.

PROF. DICKE: May I suggest that we call a halt to this, I just wanted to expose this thought.

CHAIRMAN: Good. I would like to have a free expression of how we can advise, not in any formal sense, or suggest interest to NASA in one or another of the kind of things we are talking about, including experiments you have been interested in.

PROF. DICKE: Well, I would like to reaffirm what you just said, I think the gyroscope experiments are very nice. I'm not at the moment convinced of the best way of doing this is with a low-temperature gyroscope, but I believe a good deal of thought has to be given to the various alternatives before someone commits a lot of money to it. But I think this is really very important because it does give you a crack at the first-order term in the $g_{\alpha\alpha}$ which you don't get with the red shift. In other words, if I had a million dollars to spend, I would much prefer to put it into that than to put it in a very accurate red-shift experiment, which will only give you a better value of the red shift, but will never be good enough to get the second-order terms. Another thing I feel kind of unhappy, I don't know why, about the fact that the perihelion rotation depends so strongly on one thing only - what Mercury does. I would like to see, if possible, some other way of getting information about that. I think a satellite going around the earth is a bad way of doing it. It's extraordinarily messed up by the figure of the earth. Now there is a possibility which hasn't been mentioned here at all which people have been thinking about, and that is to put out an artificial planet, not an artificial satellite, but an artificial planet, something that you throw out . . . around the sun.

PROF. BERGMANN: Very eccentric?

PROF. DICKE: Well now, there is a question here, I don't know how bad the drag of interplanetary gas is going to be on this.

If this is a factor then one has to servo this shell, that I'm forbidden to talk about, to screen the wind from it. Another possibility is not to treat this as an orbital device but merely as a surveying instrument which enables you in a very continuous accurate way to tell where the earth is relative to the device. If you have slowly varying astronomical parameters associated with this gadget and you have the earth parameters at the same time, you can measure the distance between the two with very high precision by means of microwaves. Then you not only get an orbit for the device but you get a more accurate orbit for the earth.

CHAIRMAN: This is a passive device?

PROF. DICKE: No, it's an active thing, and it will have to digest the signal it receives and retransmit.

PROF. WEBER: Well in that connection would there be any point in landing an instrument package on some other planet, say Mars, Venus, or Mercury? I would just like to ask the astronomers if you made one of the other planets active if you could do a better job on a perihelion rotation.

PROF. DICKE: Well one has the feeling that any new information of that kind would tell us very precisely how one planet is moving relative to another, even though we haven't had it over the past 200 years, to have it even for a few years, would be very nice.

PROF. WEBER: Well, isn't it easier to land an instrument package than to put a planet in orbit?

CHAIRMAN: Isn't it easier to observe an existing planet? I was raising the question with Prof. Heckman about Eros, which is an extremely eccentric asteroid. There have been various proposals to get the perihelion effect on it, the figure of merit being the product of the eccentricity with the perihelion advance per century turns out to be about the same as for the Mercury. It would be about 8 seconds of arc per century but I have talked to some astronomers about it and they say well it's such a little thing and the earth's sphere has so many big ones that it's difficult to get a definitive orbit for it.

PROF. HECKMAN: It would certainly have its advantages; of course, it's much easier to observe than Mercury. Mercury is always quite close to the sun, but on the other hand the disturbing effects of the larger planets, Venus, Mars, which could influence it on account of its very large eccentricity, would have to be worked out and they depend on other uncertainties. One has to go very carefully into the discussion of the theory of that one planet in order to see whether it's worth while to go into detailed study over many years. It is possible that Clemence in Washington is the man who should be contacted.

PROF. SCHIFF: Suppose you could land a transponder on this object so that you could do accurate radar signals, would this help in locating it?

PROF. HECKMANN: I think up till now optical locations have a precision, of a fraction of a second.

PROF. DICKE: This is far greater though, if you look at it, what you can do with radar is far more accurate than what you can do with observation. All you need is a single parameter family of numbers if you know them sufficiently accurately and the orbits.

PROF. THOMAS: Actually the perturbations on this object are very much larger than the variations in parameters.

PROF. DICKE: Remember that this is a science which is 200 years old and that it's only in the last few years that we have had computers that are able to digest enormous amounts of data and handle it.

DR. ROMAN: I'd like to go back to your artificial probe. For how long a period would this transponder have to work to give you the information?

PROF. DICKE: That's a very difficult question to answer but I would think, if I can measure the range to the thing with accuracy of say one part in 10^{10}, then I think I could get very accurate information about what the parameters of the earth's orbit are in a relatively short time.

DR. ROMAN: What do you consider a relatively short time?

PROF. DICKE: Shall I say a year? But I have no reason to believe it's a year.

PROF. SHERWIN: With what precision would such an experiment have to be made to be worth doing?

PROF. DICKE: Well we have accuracy on free-fall, I think what we are doing is equivalent to that, of about 2 or 3 parts in 10^{10}. I would guess that we could go to a new technique tomorrow if we thought it was worth while, but I feel that the most worth while thing for us to do on this experiment is to continue with improved techniques of this type and try to get another factor of 10 which means that one would be shooting at something better than one part in 10^{11}. There have been some suggestions about doing this with satellites.

PROF. DICKE: I think Leibus up there suggested a very interesting example of an experiment. You put a gold ball and, say, an aluminum ball, like this, on a dumbbell and you paint this one red and you paint that one green and let them rotate in the earth's field as a satellite. After a while it will damp down and line up either this way or that and you look to see which end is down. Do you see red or green?

PROF. SCHIFF: Along the same line I might mention something which Clemence suggested, and that is the possibility of comparing different types of astronomical surveys. If you survey by measuring angles, you get one type of information and if you survey by radar signals you get another type of information. The correlation between these depends on both the α and the γ terms of the metric.

PROF. DICKE: You no doubt know the situation with respect to the solar parallax right now. It's at an all-time low. It has always been bad and it's gotten steadily worse. Now there are a couple of accurate radar range measurements that don't agree at all with the orbit determinations.

CHAIRMAN: Prof. White do you have anything to suggest about these problems?

PROF. WHITE: My interest in this conference concerns experiments that are not of direct interest to the space vehicle program.

CHAIRMAN: Prof. Taub, would you care to assert yourself?

PROF. TAUB: Most of the experiments that are presently feasible have to do with test values of some sort or another and of these I would say the gyroscope experiment is the most interesting one, the most feasible one. I don't see any other possibility with the present techniques of getting anything on components of the metric tensor that is well enough understood so that one could hope to see the differences between general relativity and the Newtonian theory.

CHAIRMAN: I want you to also include other possible theories such as those of Nordström and Dicke.

PROF. TAUB: But I think it is important to realize that one has to distinguish between dealing with test bodies or other systems whose own gravitational fields are going to affect the situation.

PROF. BERGMANN: I think I would like to come back once more to the question of what one can hope to get out of radiation observations. It seems to me that in contrast to the more or less static or stationary experiments, including the gyroscopic experiment, that if we are attempting to detect gravitational radiation, we find out something that is first of all, not settled to anybody's complete satisfaction within general relativity, and second, something to which perhaps the choice of different theories of gravitation may be quite sensitive. The theoretical situation as I understand it, at present, is this: immediately after 1916, all the properties of linearized gravitational waves were completely and correctly described and now the one question remains whether there was anything among the closed-form solution, that is, the solution of the true field equations corresponding to the linearized solutions. That question has I think, been settled only very partially because the nearest things that might mean something physically are plane waves. These have been published in toto by Bondi, Pirani, and Robertson in the last two years. I just checked this with Joe. There is at present no spherical wave model solution that corresponds to anything that we would consider a satisfactory quadrupole wave nature. I think that most of us feel that such solutions exist but there is a difference between professional faith and knowledge, and the knowledge is at present absent. Now even if there were empty space solutions corresponding to waves, there is still a question whether these waves are produces let's say by double-star systems or not. And I think if you would take a vote some would enthusiastically vote against and most of the other people in the field would hesitantly vote in favor, including myself. Unless you want to settle the question by popularity poll we don't know. The investigation of the theory is not far enough along. It seems to me that for this reason any experiments which bear on gravitational waves would be of very acute interest to anyone who either believes in the general theory of relativity and would like to find out more about it or who questions the theory and wants to know whether it predicts the correct things. What is discouraging is that at the moment I don't see any experiment which we could recommend to NASA to do within the next couple of years with the possible exception of the one Joe Weber has in the works.

CHAIRMAN: Do you see any way in which NASA could help in this particular problem? Even in the one Weber was talking about.

PROF. BERGMANN: Well the one thing that could conceivably be done there is this unfortunate business of the nonintegrability of the affine connection, has recently come, at least, in my focus, of interest though I haven't done enough thinking about it. As I say for a very tight double-star system but perhaps one that might exist, one could anticipate effects of the order of 10^{-9}, which is very discouraging if you think of angles, but less discouraging if you think of experiments where you have two test bodies which prior to the onset of the wave are at rest relative to each other then following the passage of the wave are moving. This is still very discouraging if you consider that they had to be very far apart to show the effect. Conceivably something like this could be done with space probes and Doppler effect determination. What you would need, in principle, is to have two freely falling particles in the universe, in principle this is not a static effect; that is, it will not be contaminated by static fields like the field of the sun, etc. With two freely falling particles, and let's say one is a transmitter of light or wave radiation, the other a receiver. Now the passage of gravitational waves should have the result that the signal received by one from the other is modulated by the frequency of the gravitational wave. But, what you obviously would need before you can think of designing the experiment is some reasonable estimates of the order of magnitude of the effect and therefore some estimate of all kinds of contaminating noise. Therefore, I would like to say at this point that it is the kind of thing, be it Joe Weber, be it this effect, or something else that one should perhaps think about in the next year or so and consider whether anything could be worked out. I wouldn't say don't do what Joe Weber wants to do but do that instead, but rather try two or three things as long as they don't run into the multimillion dollars range.

CHAIRMAN: Thanks, I would like to hear Weber's reaction to that.

PROF. WEBER: Well I think there are a countably infinite number of ways of measuring the Riemann tensor. And a very large class of this countably infinite number involves free particles and the use of light. I think the calculation will almost always show that such measurements are between 10 and 100 million times more difficult to do than one in which you use particles which interact with each other by the strong interactions. Here you have resonance effects in which the energy is stored over many many cycles, and in which, once you start to calculate the fluctuations, I think you'll find that almost always this is true. That is why we were driven to things of this sort. The other thing I should like to say is that our present apparatus has so many problems associated with it that when one thinks in terms of NASA time schedules that we certainly don't have in our minds flying it. Although flying it in a satellite might help with some of the problems. I don't think this is anything we should suggest in the immediate future. I think though, that the issue of the study of the normal modes of the moon by apparatus which is landed on the moon is something which ought to be pursued of we remember that different modes have different symmetry properties. So that by the study of different modes, I think one might well be able to decide whether or not there are gravitational waves on the moon. If all the moon's modes are more or less excited in the same way then all one can do is set some sort of limits. It may be that the modes which have a quadrupole character do show a stronger excitation than the ones with the wrong kind of symmetry. This is something one ought to have in one's plans when NASA proceeds with experiments involving the moon.

DR. ROMAN: Could I ask a question about this, I'm afraid this is a field that I don't understand extremely well. But isn't the reason for going to the moon to get away from seismic activity? Why can't we do this on earth?

ANSWER: Winds.

DR. ROMAN: Now suppose there is seismic activity on the moon. Will this bother you?

PROF. WEBER: It certainly will, if the seismic activity is such as to excite the normal modes to a rather high noise level. So it's entirely conceivable that the study of the seismic activity will rule the moon out for this purpose.

DR. ROMAN: This is something that will be done in the very near future if all goes well.

PROF. DE WITT: We should be able to coordinate these proposals to detect seismic effects on the moon.

PROF. WEBER: This is the only kind of NASA sponsored experiment I can think of at the moment, other than the gyroscope type, which has any prospect at all of giving useful information.

PROF. DICKE: With respect to seismic activity on the moon, the evidence is not conclusive. We know how seismic activities are produced on the earth, we know it's connected with faulting, and faults are connected with slipping of some kind. There is absolutely no evidence of faulting on the moon. Such faults would cut across craters so that you have slipping. I think there is good reason for believing that there is no seismic activity, at least from that point of view.

QUESTION: Is it true that there is some indication of displacement of a few parallel lines?

PROF. DICKE: There is certainly at the so-called wall, but there is none of this slip-slide faulting that cuts across craters; this sliding from one part to the other which is believed by many people to be associated with defective mantle you don't see.

PROF. SCHIFF: I would like to emphasize just slightly a remark that Prof. Bergmann made about the astronomical and cosmological sources of gravitational radiation. About a year and a half ago Bondi made the remark, I don't know how well considered it was, but he said that he found that bodies moving under gravitational interaction, being accelerated for this reason, would not give up gravitational radiation; it would take a nongravitational force to produce acceleration of the masses. And if this were true this would mean that the amount of cosmic gravitational radiation is very much smaller.

PROF. BERGMANN: Not necessarily.

PROF. SCHIFF: Well anyway I can quote one specific reference on the other side because in the course of Feynman's work on setting up a classical field approach to gravitation, I made this remark to him and he actually calculated gravitational Bremstrahlung and all the radiative parameters, and he found that it was completely independent of the acceleration mechanism.

PROF. BERGMANN: There are people on both sides of the issue (more interruptions and discussion from both sides)

PROF. WEBER: As far as the cosmological aspects are concerned, even if it were true that bodies moving under the influence of gravitational interactions alone do not emit gravitational waves, there is still another possible source of separating the α term to indicate what one has. You have all of the neutral hydrogen in the universe, the hydrogen atoms grouped together, there is a possibility of gravitational Bremstrahlung. The forces there are not gravitational when they are hooked together. Also you have the possibility of radiation. Also you have possibility of radiation of plasmas connected with the stars. I just indicate this to point out that the cosmological sources may be connected with other things other than gravitational forces.

CHAIRMAN: DeWitt did you see anything that NASA could help in?

PROF. DE WITT: I would like to see this seismic thing on the moon.

PROF. BERGMANN: I would like to mention that due to the fact that one doesn't know what possible sources of gravitational radiation exist in the universe, let's say plasma versus double star system, we don't know in which frequency range to look. It might be ranging between 10^{-6} cycles and 10^{+10} cycles, and obviously it depends on which way you want to look at it. It is very difficult to form an opinion. I mention this because in the design stage, one should think of quite a number of different approaches.

PROF. DE WITT: This problem of trying to raise the issue of the eventual future; certainly in the time when we can get people and laboratories into space, then a lot more interesting things can be done.

MR. MITCHELL: What for instance?

PROF. DE WITT: For example, these difficulties raised by measuring positions relative to the stars. If you could get people up there, you could certainly get bigger telescopes up there.

PROF. DICKE: Well Martin Schwarzschild said one time when asked, "How soon can you get a balloon big enough to put both the man and the telescope up?" he said "The last thing I want to do is put the man up there. I want him on the ground because he can't do anything but shake it."

PROF. FOWLER: I think some mention should be made of the remark that the orbiting astronomical observatory might be useful in looking at a white dwarf to see whether the gravitational red shift can be solved that way.

CHAIRMAN: Any other ideas?

PROF. HECKMANN: In connection with this gravitational red shift as I mentioned yesterday avoiding the main star system like Sirius would certainly mean much higher precision of red shift even if you don't know the exact theory of surface conditions of the white dwarf. This would have a bearing on relativity.

PROF. DYER: Nothing to add.

PROF. FOWLER: There is one other point. It seems to me the possibility of development in connection with gyroscope tests is essentially a problem of the determination of the angle, and perhaps the idea of doing this with a large telescope. I didn't quite get the implication of Dicke, who said he had a big hole in the ground.

PROF. DICKE: It's a very simple thing; I can state in 2 seconds If you have a diffraction pattern this wide (draws on board) and you determine with one photon the uncertainty is this great. If I determine it with 10^{12} photons and I have no systematic errors, I can reduce this to 10^{-6} of that. That's all there is.

PROF. SHERWIN: Shouldn't this be brought to the attention of NASA that they begin to think along these lines.

PROF. DICKE: I have the impression that the astronomers could make use of certain instrumentation that the physicists know.

PROF. NORDSIECK: While measuring gravitational red shift one might ask, "Does it make sense to build a clock which you throw in the sun and it radiates back while falling into the sun"?

CHAIRMAN: This being a test of what?

PROF. NORDSIECK: Of gravitational red shift.

DR. HOCHSTIM: (Goes to board and draws diagram). We have the following system let's say this is the sun, let's say a vehicle close to the sun, this is earth, you send a signal, let's say with a frequency ν_1, it arrives on the vehicle as frequency ν_2. The idea is that you retransmit the frequency, ν_2, arrives here ν_3, also you measure on the vehicle what was the frequency ν_2, and you transmit this frequency ν_2 in a code. So technically what you have in the first one you send ν_1 to ν_2 and you have $\nu_1 - \nu_2/\nu$; the second time you have ν (lets say you retransmit) you have something like $\nu_2 - \nu_3/\nu$. Now in general, this is just function of velocity. Let's say that this is m/r which defines Ψ/c^2. This will be Ψ and this will be $-\Psi$, this is Ψ_1 and this is Ψ_2, so that I'm to find the difference. For example if the vehicle moves away from the earth radially, you find that $\Delta\nu/\nu = 1 - 2\Psi - v/c$. This reduces to the exact Doppler effect To give you some numbers the difference in the vicinity of the sun let's say I extend this formula and I find $v/c + \Delta\Psi + 1/2\ v^2/c^2$ plus terms of order $(\Delta\Psi^2)$, plus terms of order $v/c\Delta\Psi + v^3/c^3$ plus etc., etc. Now let me show you the numbers. Let's say assuming velocities 30 km per second roughly and $\Delta\Psi\ 10^{-5}$, again 10^{-4}, and this order 10^{-5}, this is 10^{-8} and this is 10^{-10} and this is 10^{-9} and this is 10^{-12} and in the vicinity of earth moon---this is vicinity of sun, this number will be roughly 3×10^{-5}, and this is 10^{-10} and this 10^{-9}. This is all that you measure here. So it seems that if you get awfully good with a system of this kind, or modification of it, you could measure higher order terms.

DR. ROMAN: How would you separate them?

DR. HOCHSTIM: As I said before, you have two equations with 2 unknowns, one is v and one $\Delta\Psi$, then you check against the formula and see how it's agreeing. Of course you could make it continuous. You could have the earth transmitting the bit of information.

PROF. SCHIFF: I think Nordsieck had a special case of this and perhaps a somewhat simpler method. You could use the earth as one of your vehicles and then the other thing that's going to the sun is the second one and by accumulating signals you could get the course and also the frequency. I think the $(\Delta\Psi)^2$ is somewhat too big. As I recall $\Delta\Psi$ at the surface of the sun is 10^{-6} so with the squared term you get 10^{-12}.

CHAIRMAN: I computed once for a less serious purpose the difference in proper time from a parabolic satellite launched as follows. Here is the sun, here is the earth, launch a parabolic satellite around the sun so that it comes back just in time to reach here. I computed the difference in proper time (I forget what it was) something like 0.6 of a second. It comes back about 4 months after the thing had been launched. Any other comments? Jones, you are responsible for this gathering.

MR. JONES: I would like to thank everyone for coming. Certainly if NASA is able to receive such excellent consulting service, we won't have any trouble.

CHAIRMAN: I haven't attempted to call on everyone here. I looked around and saw some sitting on the edge of their chairs and decided they were the ones who wanted to say something, but I'll be glad to hear from others.

MR. MITCHELL: We didn't discuss this manned aspect but are there any useful things in connection with Apollo, which is scheduled to carry a man to the moon, that you can do on the moon. Now we are talking about the latter part of this decade.

COMMENT: If you could get a man on something like Eros where there is negligible gravity, that might be better for several reasons than the moon for the gyroscopic experiment.

MR. JONES: It occurs to me that attaching oneself to an astronomical body would have a bad effect. Really, in most cases the other body does more harm than good. What is wanted are bodies with a large moment of inertia but with a small attractive field.

DR. ROMAN: Large moments of inertia are real nasty.

CHAIRMAN: These are thrills that I think are rather precarious. When I read about frogmen going down 60 feet and coming up with the bends I wonder how much we could stand.

PROF. NORDSIECK: I hope that all the people interested in the gyro experiment could keep in touch with each other.

PROF. DICKE: I would also like to say that anybody else who is interested in putting up a time-keeping satellite . . .

CHAIRMAN: Leonard I didn't call on you.

PROF. SCHIFF: I think Nordsieck has said this should be coordinated effort with the gyroscope.

CHAIRMAN: Well I understand in fact that you are interested in preparing a proposal from that and therefore it seems to me, in view of your good contact, NASA would be the proper place to handle it. A sort of exchange of information.

MR. MITCHELL: There are a number of things in this connection demonstrating that you can make a gyro with an accuracy of this order, so that you can conceive of doing this in a satellite. This is at least one phase which I think you should consider. You can only do so much in the laboratory.

PROF. NORDSIECK: What you suggest isn't so easy, all you can establish is that an instrument will operate, but you can't establish that it will do what you want.

MR. MITCHELL: That's right but one of the first things you are going to have to establish in space is that you have a gyro which has an accuracy of at least 3 orders of magnitude more than the present gyros.

PROF. NORDSIECK: You can't establish that it has that accuracy by a probe. You have to have it in free-fall for a long time.

DR. ROMAN: You may not be able to establish to 10^{-14} but you have to be able to establish 10^{-11} or 10^{-12}.

PROF. NORDSIECK: It has to stay uncaged for months or at least weeks.

MR. MITCHELL: You can only establish that it will in fact operate and a lot of the other problems. Well Prof. Pound thought he had all the things that could go wrong with the experiment and still something happened that he hadn't considered, the temperature effect was a lot more than he thought it would be.

COMMENT: It would make more sense to put it piggy back on some other satellite where it would be under zero g for a long time.

PROF. NORDSIECK: As far as I know that is the only way to tell if you have a reasonable instrument. I don't think you can tell it in the lab or in a short time in space. This is a bad feature of the whole thing.

DR. ROMAN: It does not have to be within a satellite space? Suppose you do ride it piggy back.

PROF. NORDSIECK: It has to be in free-fall for 3 months.

DR. ROMAN: Yes, but suppose you ride it piggy back. Can't it be in a satellite which, say is earth oriented or turns from one part of the sky to another or something of this sort?

PROF. NORDSIECK: I wouldn't say. Could be.

COMMENT: I think you could not measure anything like the accuracy we are talking about under the circumstances, but you could take a reading when it happened to be in the vicinity of its original orientation. You could take a reading and find out what sort of operation it had been carrying on: You could take advantage of a fixed inertial direction.

PROF. THOMAS: It seems to be agreed that the gyroscope has the best merit. I was wondering to what extent such an experiment could serve to distinguish between the various alternative theories - Birkhoff, Dicke and others.

CHAIRMAN: Dicke, I wish you had heard that. The question was raised as to whether the gyroscope experiment would help to decide between alternative theories. He mentioned Birkhoff's, yours, etc.

PROF. DICKE: It would certainly help to distinguish between general relativity and this one I describe. There is no difference if you stick only with the red shift, but there is a difference in this experiment.

QUESTION: How much?

PROF. DICKE: Well it depends on what choice you make for that ω parameter. I'm sorry but I don't have a number real handy, but I would guess that it's of the order of 3 to 5 percent.

CHAIRMAN: 3 to 5 percent? Well 6 or more; it seems to me more.

PROF. DICKE: You tell me what the hot formula is and while you are talking I'll compute it. What's that you were mentioning?

CHAIRMAN: $\alpha + 2\gamma/2$.

PROF. KING: I was just going to ask him if he was going to carry the red shift to the first order and then stay there.

CHAIRMAN: To this question. It seems to me that the Birkhoff theory would give no distinction; this would be the same as general relativity.

DR. ROMAN: I think one thing should be mentioned and that is the difference between the radar result and the standard result should not be worried about too much. There are two values in the astronomical unit which, well, there are 3 that have been derived in recent years if you include the one that is very recent, but the one by Spencer Jones and one by Robert are both extremely careful values which disagree by many times their probable errors, and the radar result fell between.

PROF. DICKE: Which is the radar result?

DR. ROMAN: The radar result . . .

PROF. DICKE: You mean the recent one.

DR. ROMAN: The recent one, has been determined by the 1958 result, which is superior. Leave out the Russian value, and the MIT and JPL values seem to be in complete agreement.

PROF. HECKMANN: There is one other point which should be considered to be proved by a telescope and probably by a different telescope. This is the effect of light bending around the sun. At present you need a total eclipse, but if you are sufficiently high, you don't have to wait. You need a special camera, not a big telescope, but a good and well-designed camera. You can get an arbitrary number of data points. You should do it at different seasons so that the sun is projected against different backgrounds of stars, and in that case you can easily overcome Freundlich's criticism, which I don't believe in.

CHAIRMAN: Has this been discussed?

DR. ROMAN: No it hasn't; I think it's a good idea. Again there are a number of technological problems.

QUESTION: Do you plan to recover photographic plates from a satellite?

DR. ROMAN: Well, for the ones that we are planning now, no. However, we are, particularly with the oncoming of the Apollo program, beginning to think of the possibility of the recovery of plates.

PROF. HECKMANN: But it's much lower in weight, it's not such a big and heavy thing.

DR. ROMAN: But you need the pointing and that's what runs your weight up. It isn't the optical instruments.

PROF. HECKMANN: You can make the pointing automatic so that the telescope points itself.

SUGGESTION: Telescopes can be connected with recovering photo plates at different seasons.

CHAIRMAN: The readout would be pretty rough.

MR. JONES: It sounds as if this experiment could be performed with the OAO.

CHAIRMAN: Except then comes the question of the plate recovery. Then you would have to read it out.

PROF. THOMAS: You have to measure pretty accurately the positions of several stars on the plate.

PROF. DICKE: I have the number. 10 percent less.

PROF. HECKMANN: It would be much easier if you could have a man to change the plates. We must bring down the plates.

DR. ROMAN: This is what I had in mind. We are just now beginning to think about the possibilities of a man going out to a telescope, getting some plates and bringing them back. (discussion among several people about man's bringing the plates)

PROF. KING: I had the feeling that there was not much point to atomic clocks in a satellite. However, Hochstim's and Nordsieck's suggestions make me feel that it is not so far out of the question if you start planning for second order terms, that is 10^{-12}. We would like that a little better; hitherto, it's been out of the question.

PROF. SCHIFF: This is the only nonlinear thing besides the orbit precession.

CHAIRMAN: Weber, do you have anything you would like to add?

PROF. WEBER: No.

DR. ROMAN: Well I want to add my thanks to those of Mr. Jones, to all of you for coming and participating to the extent that at least to me it has been very interesting and very valuable. We at NASA, I think you know, like to call ourselves a service organization and I think as far as a scientific community is concerned, we are to a large extent. We would like to try to provide the capability to do the scientific experiments that you people think are worth doing and, in spite of the crack made about consultation without fees, I think it's only in meetings such as this that we have any way of finding out what experiments you do think are worth doing. To add to that I want to thank Mr. Jones for the work that he did in organizing the conference and to Mrs. Drew for being so patient in taking down so much difficult technical conversation and to the others at Ames for the helping with arrangements; to Dr. Schiff, for arranging a pleasant meeting place for us and the local arrangements here for meals and housing; and last but not least to Dr. Robertson for the arranging of the scientific aspects of the meeting and for being such a forceful chairman and keeping us on schedule. Now if I may add one bit of salesmanship. In order for a conference like this to bear fruit, we have to have some activity from you people. We are too few at NASA to go ahead and carry out all this work which has been suggested. We would be very happy for you people to think about doing some of the things that look like they are worth looking into, at least to the feasibility of them in greater detail. We will welcome proposals; we won't promise to fund them all. We have budgetary limitations like everybody else but I would like to hear from you. I think our only ground rules are, first, they should have some relation to the space program and, secondly, you should not try to do in space anything that you can do as well on the ground.

CHAIRMAN: Prof. Schiff has stuck his neck out by saying he is going to prepare a summary of this conference.

PROF. SCHIFF: I got roped into this by having to give a talk at the American Rocket Society which meets at Stanford in about 10 days or so.

CHAIRMAN: I think we owe you a vote of thanks.

DR. ROMAN: I might add one more word along that line. If the stenotypists notes and the tapes can be edited satisfactorily, we will try to publish the proceedings of this conference. Now for that we are going to need a great deal of cooperation from the speakers. I think this is obvious, so most of you will be hearing from me one of these days, and I'll be asking will you please look over what you said and see if what we put down is really what you intended to say.

ADJOURNMENT: 4:20 P.M. Friday.